

AN ALCOHOL TREATMENT EVALUATION WITH A SPECIAL FOCUS
ON AFFECT BALANCE

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Ph.D

University of Edinburgh

1990



TO

AGNES, DERMOT, MAURA and BRIAN

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Acknowledgements

I wish to acknowledge a profound debt to my two supervisors Dr Alex Robertson and Dr Stewart Asquith and express both my gratitude and appreciation for the scholarship and support which they offered unstintingly. Without these gifts and their forbearance this thesis would not have been possible.

I owe a debt of gratitude to the individuals who are the subjects of this research who remain anonymous in the text, but are remembered by myself. I wish them all the very best of good fortune.

An enormous debt is owed to my four friends Mr Bill Campbell, Miss Irene Kennaway, Mr Neil McElhoney and Mr James Dalton. Without their skills in computing and word processing and their support the completed thesis would not have seen the light of day.

Also, for statistical advice, I am indebted to John Duffy of the University of Edinburgh and Jim McCall of the University of Glasgow.

Finally, my thanks go to the University of Edinburgh for its acceptance and provenance.

ABSTRACT

In this thesis the literature on Alcohol treatment, evaluation and related issues is reviewed from a historical perspective. Conclusions were reached about both the efficacy of treatment and treatment evaluations. The possibility of a new source model of treatment was considered.

Patient data on outcome was analysed on a before and after basis. It was found that the successful outcome group exhibited much the same characteristics as the rest of the sample at phase one, such that it was not possible to extract a discrete profile of completers. It was further noted that that successful outcome group displayed similar characteristics to other such groups reported in the literature, social class being a prominent exception.

Process data were analysed to observe the pattern of change over the study period. This pattern tended to conform to common sense views of the recovery process held by clinic staff. The pattern was predominantly linear with a disruption occurring around the three to four months period, thereafter the scores continued in a linear fashion. Correlations amongst the scores were noted.

The concept of affect balance was introduced as a clinical, methodological and theoretical construct that has value for treatment evaluations. A thirteen life domain scale was constructed to operationalize the concept of affect balance and its properties were discussed.

Process data and outcome data were analysed with affect balance using a variety of statistical techniques to observe the importance of affect balance for other variables. It was observed that affect balance correlated with variables thought to be important in alcohol treatment evaluation and provides a model which helps researchers make sense of the treatment process. Confidence in the results was strengthened by the rejection of the null hypothesis by multivariate methods.

Trend tests of the change scores across the study period indicated the presence of a significant trend in the desired direction for all the psychological variables and Affect Balance. In the case of social and programme variables (subject's perceptions of the treatment programme) four out of a total of seven change scores exhibited a significant trend.

Further analysis focused specifically on Affect Balance. Within individual change scores study variables were generated to observe their correlation with Affect Balance. Affect Balance did not correlate with any of the Drinking related variables and only one of the social variables (Disturbed Behaviour Present). The first result was to be expected, the second much less so.

The psychological variables were subjected to an analysis of covariance. Affect Balance was found to covary across the study period with all but two of the psychological variables. The two exceptions were obsessionality and hysteria.

Additional analysis employing stepwise multiple regression indicated that Affect Balance accounted for moderate amounts of variance in five nominated outcome variables at phase one (B1), and failed to predict a sixth, wincode(amount of treatment received).

Attempts were made to ground the concept of Affect Balance in the theoretical literature on Emotions, and its wider applications in the Social Science research was discussed. Further research objectives were noted.

LITERATURE REVIEW - THE EARLY YEARS

In order to give a historical perspective on work within the field, this chapter will present a chronological review of the literature on alcohol treatment outcomes, and material ancillary to this. It is hoped in this way to provide the proper context and backdrop to the main theme of this thesis: namely, the concept of Affect Balance and its value for alcoholism treatment evaluations. The chapter will discuss the development of alcoholism treatment programmes from the point of view of outcomes or of treatment efficacy, and where appropriate, relate these issues to their relevant methodological and ideological contexts.

What follows is not intended to be an exhaustive formal literature review of alcoholism treatment; in my opinion a sufficiency of such reviews already exists. References in the review list give a sampling of outcome review studies spanning five decades, and are inclusive of all the major treatment reviews of which I am aware. (These are appended in a separate review list after the references cited at the end of this chapter.)

If a further reason were required for not advancing yet another review-listing, it would be that such an endeavour necessarily would be incomplete or partial. This is so for three reasons:-

1. The absence of adequate funding.
2. The time involved.
3. The difficulty of securing an appropriate organisational

setting.

Moreover, one's intuition is that such an undertaking would be a largely redundant exercise, arguments about meta-analysis apart, because of the prevailing and majority theoretical view within the alcoholism treatment field at the present time. That is to say, it is more likely than not that any critical conclusions relative to the body of work done would concern norm-standardisation and incommensurability.

At a conference on alcoholism at the Institute of Psychiatry in London in 1979, an American psychiatrist, M.L. Schuckit, reading from a paper entitled "Charting what has changed", commented as follows :- "In fact, treatment evaluations carried out in the 1960s and 70s had not changed much from what was noted in one of the original syntheses presented by Voegtlin and Lemere". He further went on to say, "We still have a variety of approaches all of which work (or fail to work), to about the same degree". The closing sentence of Schuckit's paper reads, "We are now in a state of flux, we have not accomplished nearly as much as any of us would like, but the changes of the last decade have been more than anyone realistically would have expected in 10 years" (Schuckit 1980).

The syntheses of Voegtlin and Lemere which Schuckit used as a base-line were done in 1941, and represented a review of all the studies that reported treatment results with alcoholics between 1909 and 1941. A quarter of a century later, Hill and Blane in their frequently cited review of alcoholism treatment outcome studies began with a quote from

Voegtlin and Lemere (1942): "The most striking observation is the apparent reticence with which English-speaking psychiatrists have presented statistical data concerning the efficacy of treatment. With the exception of two authors, the medical profession at large is unable to form any sort of opinion from an examination of the literature alone, as to the value of conventional psychotherapy in the treatment of alcoholism in this country or in England". They went on to comment, "More than 20 years later, we find that any such apparent reticence in the presentation of statistical data has gone, there is now little reluctance on the part of English-speaking psychiatrists to report statistics. Importantly however, and unfortunately we can still perhaps agree with Voegtlin and Lemere, that we are still unable to form any conclusive opinion as to the value of psychotherapeutic methods in the treatment of alcoholism".

Hill and Blane (1976) reviewed 49 studies published in the United States and Canada between 1952 and 1963 which reported on the evaluation of psychotherapy with alcoholics. They conclude that "these studies fail to live up to their potential for contributing to knowledge because of a failure to meet many methodological requirements for the conduct of evaluative research". Indeed, it seems that what was planned as an evaluative project by Hill and Blane inexorably changed into a critique of research methods: that is, the methodology and whole philosophy concerning outcome research.

The reviews undertaken by Voegtlin and Lemere, and those of Hill and Blane are the first two such major reviews

to be encountered in the alcoholism treatment literature. In terms of their coverage of individual studies included for scrutiny, they number 259 and span the years from 1909 to 1963. It would of course be preposterous as well as false to suggest that in every case the individual studies had no value at all for their authors or for those who read them. Both pairs of reviewers comment that many of these studies made no attempt at rigour, and that they were purely descriptive accounts of work involved in each case. Nevertheless, it is difficult to remain unaffected by the thought that a huge amount of work, over half a century, added little or nothing to our knowledge of Alcoholism Treatment.

The Voegtlin and Lemere and Hill and Blane reviews made independent catalogues of complaints concerning the methodology of individual studies. The most serious of these was that concerning the failure to make use of any sort of control group during the implementation of research strategies. Even reports such as "no data are reported" were encountered (Voegtlin and Lemere). Moreover, no attempt was ever made to define or even to contextualize the concept of "cure" in any of the studies quoted by these authors. Nor indeed were the putative research-goals and acceptable outcome-criteria properly specified in many instances. The criticisms made by Hill and Blane are almost too numerous to list. In addition to those mentioned above they also noted inadequate sampling-methods, unreliability of measuring instruments, and problems such as poor follow-up techniques. Hill and Blane also remarked on the fact that since the

earlier-reviewed psychiatrists had "lost their reticence" for reporting statistics, this only appears to have put a heavier onus on current reviewers insofar as "more time must be spent in trying to read a table than in reading the entire paper: if one purpose of graphic methods of presenting data is simplification, this represents a gross misuse of descriptive statistics". Hill and Blane conclude their review with this advice: "For the person or agency planning to embark upon an evaluative study, there are a number of published sources that may be of use in avoiding the pitfalls described in this review. Wherever possible, evaluative research should be designed and supervised by a person whose interest and training are primarily in research". Perhaps this closing comment by Hill and Blane, both psychologists, is sober advice to their readers, gleaned as it is from their work in reading the productions of people who in the main were psychiatrists. With the benefit of hindsight one can, however, perhaps see here the shadows of an emerging troop of psychologists who were to compete with their medically trained colleagues for dominance of this field a decade later.

I have lingered somewhat over these two early reviews because they are important historically as a benchmark for later work. I have noted the impossibility of drawing firm conclusions due to the lack of poor or inadequate design and analysis - the problem of within- and across- study comparisons- the pre-eminence of psychiatry with its individualistic approach, epitomised by the use of the term psychotherapy and also by the entry of psychology into the

field.

With regard to the dominant treatment ideology of the period under discussion, it is important to recognise that the traditional disease model, with its prescription for total abstinence, was at the time the prevailing one. (Alcoholics Anonymous 1955. Heather and Robertson 1985)

Alcoholics Anonymous was founded in 1935 with a fierce commitment to the disease theory of alcoholism, and to total lifelong abstinence as the only possible treatment response. It enjoyed a meteoric growth, and in the United States became the main pressure-group arguing for services to be provided for alcoholics. In Britain it did not grow substantially until the 1950s. Alcoholics Anonymous impressed the disease-conception of alcoholism on the public consciousness and influenced treatment provision for decades, and it still continues to do this even though it is no longer the sole treatment provider (Leach and Norris 1977). In 1951 Alcoholics Anonymous claimed that about a quarter of their membership achieved fairly immediate successes, and that about 60% of the remainder who returned to their meetings were eventually successful. These claims were made in the famous "Big Book" of Alcoholics Anonymous which is referenced above. Such claims were completely unsubstantiated by valid data but nevertheless were widely accepted at that time in the 1950s when most hospital treatment consisted in a period of in-patient detoxification followed by attendance at hospital-based Alcoholics Anonymous groups.

In 1960, Jellinek produced his influential literature

review in a book called the Disease Concept of Alcoholism (Jellinek 1960). In this work he suggested that there were a variety of types of "alcoholisms", one of which, (gamma) would correspond to the notion of alcoholism espoused at that time by Alcoholics Anonymous: namely, that an alcoholic was an alcohol addict. The World Health Organisation had made the distinction between alcohol dependence and alcohol abuse (WHO 1952). This early distinction, made both in Jellinek's book and by the WHO report was effectively lost when the abstinence lobby adopted Jellinek as a symbolic patron, disregarding the latter's carefully stated caveats and particularly his proposition that his results should be treated only as working hypotheses. Instead of this, any form of problematic drinking became "alcoholism"; at least outwith the rather sparse alcoholism research community.

A major undercutting of the dominant ideology of the time concerning alcoholism occurred in 1962 when Davies published a paper on resumed normal drinking among alcoholics. (Davies 1962) This paper reported "normal" drinking among seven alcoholics out of a cohort of 93. These seven "resumed normal" drinkers were monitored in a follow-up study for periods that varied between 7 and 11 years. The appearance of Davies' paper caused shock-waves throughout the alcohol research community and evinced denials and expressions of outrage among the many journal correspondents who reacted to it. The responses to Davies' paper were generally couched in terms which reflected a belief in, or adherence to, the views of Alcoholics Anonymous regarding total abstinence. In

retrospect one can view Davies' paper as a progenitor of a new movement in the alcoholism field. Mark Keller, the then editor of the Quarterly Journal on Studies on Alcohol, reflected upon the circumstances of the publication of Davies' article in 1978: "I remember the words of one of my editorial referees, the clinical director of a great research foundation. He wrote, 'Dear Mark, this is terrible, but you have to publish it'. I had of course known that is what I would be told, and of course I did my duty again and published it. I also published about 18 comments on the article. Some tried to explain it. Some tried to explain it away." At any rate the article by Dr. D.L. Davies opened up the question.

Keller went on to say that shortly after the Davies article, he published another similar one, by Kendell (Kendell 1965). Kendell also worked out of The Institute of Psychiatry, at the Maudsley Hospital, London. His article reported on four more alcoholics who returned to normal drinking and were followed up from three to eight years after stopping their problematic drinking. Subsequent to these papers by Davies and by Kendell, several reviews of outcome studies reported on the phenomenon of resumed normal drinking in their study-pools (see review list, Gerard and Saenger 1966; Lloyd and Salzburg 1975; Sobell and Sobell 1975; and Pattison et al 1977). These findings concerning "resumed normal drinking" were of course impossible to reconcile with traditional conceptions of the nature of alcoholism which, in the 1950s and 60s, were based on a crude disease conception that entailed the notion of an ineluctable progression of the

disease should the alcoholic not abstain completely and permanently. The Sobells (1978), have suggested that traditional concepts of alcoholism which have not been supported by empirical findings, are essentially "folk science" (Ravetz 1971) and that the dominant ideology of the period being considered in this section of the literature review did not derive from a single origin but from a composite of views, culled primarily from Alcoholics Anonymous, Jellinek's disease concept, and public interpretations of these and other ideas. All of these latter kinds of conceptions about alcoholism had, it is fair to say, been developed to meet humanitarian and socio-political needs rather than to represent a furtherance and concretisation of truly scientific explanations, theories, and descriptions.

The Sobells (1978) in their discussion of Jellinek's work, comment that, "even though the disease-concept . . . served to organise and give medical credence to the scientific and naturalistic observations of his day, he (Jellinek) readily acknowledged that his formulation lacked a demonstrable empirical foundation". Jellinek "had acknowledged that his primary intent in advancing the disease-concept was to influence both contemporary medical practice and socio-political process". Empirical research into alcoholism and its treatment emerged in the 1960s largely as a laboratory-based enterprise primarily undertaken by psychologists. Encouraged and perhaps even motivated by the papers of Davies, Kendell and others, long-held conceptions about alcoholism such as loss of control, craving and process-

irreversibility were, at this time, beginning to be put to the test.

In addition, a new tradition of community-surveying was coming to the fore as computer hardware and the associated "new technology" became increasingly available to researchers. It is perhaps salutary to note that prior to the early 1960s, practically every piece of aetiological, epidemiological, and treatment-based research into alcoholism, was confounded by sampling-biases. This state of affairs only began to change after the work of Cahalan, who did the first cross-national survey of drinking behaviour in the United States (Cahalan 1970). Prior to this, all alcoholics studied in research programmes had, in one way or another, been specially selected and labelled in hospitals, in clinics, on "skid row" or in prison, etc. By 1963, the American psychologist David Pittman was writing about the need to utilise sound evaluative practices in the implementation of treatment programmes (Pittman 1963). In 1967 Blum and Blum, in a textbook on alcoholism, suggested that funding and research should be directed at the problem of therapists' resistance to evaluation. They quote lists of good methodological practices in the design of outcome studies which, by the time their book had been published, were becoming widely accepted. Generally, outcome studies began to provide a profile of treatment which approximated to what Frederick B. Glaser described as "the rule of one third". In discussing the matching of clients with treatment, Glaser says, "uniform approaches to complex problems rarely produce satisfactory results in more than a

small proportion of cases. A large proportion will either remain unchanged, or will actually worsen. This general phenomenon has informally been enshrined as "the rule of one third" (Glaser 1980).

Gerard and Saenger's (1966) substantial evaluation studied eight quite different treatment programmes in the United States following up some 800 individuals after an interval of one year. Of their original sample, 602 were traced. At the time of follow-up, 13% were institutionalised, 2.4% were dead. Gerard and Saenger concluded that, on balance, clients benefited from treatment but only to a limited extent. Their results showed that only 35 individuals had learned to control their drinking one year after receiving treatment geared toward abstinence. These 35 individuals managed to control their drinking despite and not because of their treatment. Although the treatment programmes studied by Gerard and Saenger were quite different from each other, this study concluded that no one programme or group of programmes was any more efficacious than others of those examined.

In 1953 Lemere undertook a retrospective outcome study on 500 alcoholics none of whom had received any treatment. Their average age at death, he found, was 55 years. Prior to death, it was noted that a not inconsiderable group had either stopped or modified their drinking habits. It was, however, also clear that 28% had drunk themselves to death, 3% had become abstinent, 10% had regained control over their drinking, and a further 29% had remained chronic alcoholics up to the time of their death (Lemere 1953). Since Gerard and

Saenger had used no controls in their study, the Lemere finding that spontaneous remission occurred in 10 - 15% of cases, if controlled for in their study as corrected summary data, would leave them very little to say concerning the effectiveness of all of the treatment regimes they had examined. Offsetting this to some extent, Gerard and Saenger did note that individuals in the best outcome-category they had studied, had tended to be those receiving most treatment as measured by the number of visits they had made to treatment centres. Gerard and Saenger also listed factors that were independent of outcome such as age, race, sex and level of education at intake. The use of Antabuse (Disulfiram) seemed also to be a good predictor of favourable outcome as indeed was social stability, howsoever measured. Of some interest in addition to all this was the finding that seeing a general practitioner appeared to expedite favourable outcomes, more so than being treated by a psychiatric specialist in alcoholism. On the face of it, this seems quite puzzling. To make sense of this latter finding one might consider the nature of American psychiatry at the time the review of Gerard and Saenger's pool of studies was undertaken, it being very likely that most psychiatrists involved were guided by psychodynamic principles. This being the case, they would, in the course of their endeavours, try to influence intra-psychic phenomena, whereas the General Practitioner would have been just as likely to have busied himself or herself with the more practical and mundane issues associated with stopping drinking and with achieving more efficient social functioning. In

essence, the approach of the General Practitioner was more akin to that of a modern alcoholism treatment specialist than was the psychodynamic approach of psychiatrists.

Gerard and Saenger's findings concerning social stability can further be illuminated by reference to corroborative studies. 15 years prior to the Gerard and Saenger work, Strauss and Bacon (1951) in a classic paper demonstrated from a study of 2000 male alcoholics that the most successful outcomes in their cohort related to four measures of the social stability of their subjects. These measures were :-

1. Having been in employment for three years or more.
2. Being resident in the same area for over two years.
3. Living in one's own home, or with a relative/friend.
4. Being married and living with one's spouse.

The issue of social class and social stability will become more apparent as I proceed to adduce more and successive reviews. At the time it was written, the Strauss and Bacon paper had a substantial impact. Many years afterwards, the sociologist Robert Strauss described how, at the time, he and his colleague Bacon had been astounded by their results, indeed Strauss remarked that he and his co-worker had expected to be encountering skid row, or near skid row types, such was the stereotype of the alcoholic in the 1940s. This was the first major study of its kind on the then newly-created treatment centres. It might be said that it was indeed the first "head count". So impressed were Strauss and Bacon with the social stability data they had collected that

they decided to call their paper "Alcoholism and Social Stability".

In connection with the implied ideological handicaps of the Gerard and Saenger review, it is as well to bear in mind that at the time of that particular review, and for two decades previously, there had been a major research agenda to locate and to delineate the so-called "alcoholic personality". Perceived traits elucidated from psychological personality inventories, usually the Minnesota Multiphasic Personality Inventory (M.M.P.I.) were claimed to distinguish the alcoholic from the rest of the non-alcoholic population, the idea here being that there was a basic personality-structure characteristic of the alcoholic and other sorts of addict. Such an addictive personality theory did not get off the ground in any real sense. Prior to the emergence of behavioural sciences in the vanguard of alcoholism research, millions of dollars were invested in efforts to pin-point and to dissect this elusive alcoholic personality. However, it was not to be; the "beast" was never found. It was not and is not possible to demonstrate that a particular cluster of personality traits indicative of an addictive personality precedes the onset of an addiction. A sounder strategy was to look for an addictive profile that was absent in individuals who had no addiction, but this has not been successful either and the hunt has almost, but not quite, been abandoned. Jellinek (1952) never accepted the concept of an alcoholic personality; to do so would have been to run counter to the alcoholism movement whose objective was to enlist public and

governmental support for alcoholism treatment services. Today a diminishing number of clinicians, counsellors and researchers can be grouped alongside the millions of members of Alcoholics Anonymous who espouse these kinds of views. However, during the period covered by the Gerard and Saenger review, such an outlook elicited encouragement in the United States, from psychiatric alcoholism specialists, especially those with psychodynamic perspectives (Apeldorf 1981, O'Leary et al 1981).

In the mid 1970s, a number of treatment reviews appeared (see review list). Crawford and Chalupsky (1977) reviewed the treatment outcome literature for the four years between 1968 and 1971 inclusive. Their pool consisted of 40 studies which they systematically and methodologically mauled. Their conclusions were familiar: "Most studies were both scientifically and practically unproductive". However, outcome research was, by the time that Crawford and Chalupsky wrote, improving, and further reviews during the 1970s addressed studies that had considerable methodological improvements vis-a-vis most of those carried out hitherto. Sadly, the two prevailing impressions still gleaned by researchers tended to be, on the one hand flawed methodology, and on the other, poor recovery-rates within and across studies. These "recovery-rates" sometimes reached 35% but were generally much lower than this. It is significant to note that at this time studies began to be compared with "recovery without treatment" (Smart 1970). Smart said the following about "insight-therapy": "Insight therapy of

various sorts is still the most widely used method of treating alcoholics; and still nobody has proved whether or not it works, nor what makes the difference between therapy that works and therapy that does not work". Smart had reported that between 1951 and 1961, 2% of his study-population in Ontario had recovered without treatment; other researchers put the figure for spontaneous remission much higher than this. Baekeland adduces a range between 2% and 15% (Baekeland 1977) depending, *inter alia*, upon the social class and social stability of the particular individuals studied. The issue of spontaneous remission, coupled with increases in sophistication in design of studies generally in the 1970s, appeared to be a direct consequence of the parallel developments that had taken place 10 years before in psychotherapy process and outcome research.

PARALLEL DEVELOPMENT IN PSYCHOTHERAPY

Psychotherapists seem to have been spurred on to the undertaking of serious outcome research by Eysenck's classic review paper written in 1952 (Eysenck 1952). In this paper, Eysenck claimed that it was not possible to return a verdict upon the effectiveness of psychotherapy because of methodological flaws inherent in most, if not all psychotherapeutic outcome research. His basic strategy was to compare certain of the published outcome figures with a calculated spontaneous remission rate. This computation seemed necessary to Eysenck because none of the 24 outcome studies that he had examined, totalling 7,000 individuals, had

used a control-group. His spontaneous remission rate was constructed from data taken from two previous outcome studies, those of Landis (1937) and Denker (1943). His estimate was for neurotic patients: that two out of three of these would be expected to "recover" within two years, with or without treatment. In his paper, Eysenck reported a 44% recovery-rate for patients treated by psycho-analysis, for those treated eclectically, the rate rose to 64%, but highest recovery rates of all were those treated by their general practitioner, in which case the recovery rate rose to 72%. There thus seemed to be an inverse relationship between recovery rate and manner/extent of psychotherapy received. In this paper one can see the origin of the "rule of one third" so descriptive of alcoholism treatment. Eysenck concludes "they fail to prove psychotherapy, Freudian or otherwise, facilitates the recovery of neurotic patients. They show that roughly two-thirds of a group of neurotic patients will recover or improve to a marked extent within two years of the onset of their illness, whether they are treated by means of psychotherapy or not".

Eysenck extended his survey of studies again, in 1960 (Eysenck 1960). He included the now much-quoted Cambridge-Summerville study (Powers and Witner 1951). In this longitudinal study of delinquency, the treated group did worse than the matched controls notwithstanding avowals from clients and their associated therapists that they, (the clients) had benefited from treatment. Eysenck's conclusion to his 1960 paper was more pessimistic than that taken from his previous,

1952 endeavour. As an aside, it should be noted that the Cambridge-Summerville youth-study provided data on the natural history of alcoholism, and that McCord (1978), followed up 500 of the original 650 youths after a period of 30 years, a most extraordinary feat. McCord claimed that the programme had failed to achieve a single major objective and that there was "some evidence of negative side-effects". These included, on her estimation, alcoholism and crime - again the treated youths were somewhat worse off on all measures as judged against their "untreated" counterparts.

Eysenck (1969) again stated his views concerning the effectiveness (or lack of it) of psychotherapy, claiming that it was, "indisputable that psychotherapists and psychoanalysts have failed to do any of these things which would have resulted in enhanced remission-rates, and until they have been done, I find it difficult to see how any doubt can be thrown on my conclusion that published research has failed to support the claims that it has made" (Page 100).

One of the first review attempts that was made to counter Eysenck's criticisms, was that done by Bergen (1966). Bergen reviewed controlled studies that had been undertaken as a response to Eysenck; but nevertheless Bergen was, on the evidence obtaining, still unable to demonstrate that psychotherapy was or had been effective. Generally, psychotherapists were enraged by Eysenck's critical assertions, (this indeed is still true) but his criticisms undoubtedly acted as a catalyst and motivated psychotherapists as a whole, seriously to consider and to implement properly

conducted research programmes.

A conference of psychotherapists was convened in 1958 where it is fair to say that many of the "nuts and bolts" issues of outcome research were discussed. The proceedings of this conference were published (Parloff and Rubenstein 1959). Parloff and Rubenstein had commented at this conference that outcome research was more or less universally scorned as being "applied" as opposed to two other areas of import under discussion, process and personality theory, which were regarded as more lofty species of basic research. By 1961, at which juncture Hans Strupp had convened a second conference, the outcome versus process studies conflict had all but evaporated (Strupp and Luborsky 1962). Strupp and Luborsky commented that the greater sophistication of the then recent outcome studies had thus made these more "respectable". Research methods certainly did become more sophisticated and alcoholism research benefited from the developments in psychotherapy: however, just as the methodology was becoming more sophisticated, so were concomitant research problems. By 1971 Bergin was able to conclude that psychotherapy was helpful "in some cases" (Bergin and Garfield 1971).

In 1971, Truax and Mitchell reviewed the literature on how different sorts of therapists were thought to operate, despite differing theoretical orientations. Truax and Mitchell claimed in their study that despite their disparate theoretical perspectives, what they (the therapists) appeared to do in front of the clients was very similar. Truax and Mitchell argued that therapists' personalities are more

important than their techniques insofar as successful outcome was concerned. Good therapists are said to be warm, empathetic and genuine; they proffer help quickly and constructively (Truax and Mitchell 1971).

At this point, a brief mention should be made of the most widely quoted and influential review of comparative outcome studies in the psychotherapy literature, and one which is often referred to in the alcoholism literature, that of Luborsky and colleagues (Luborsky et al 1975). This review goes a little beyond the period under discussion in this section, viz. from 1900 to 1970. Luborsky et al dropped methodologically crude studies from their review pool and concentrated upon those which used controls and genuinely non-psychotic clients. They culled their study-data from a twenty year period. Each study was assigned to a review category for analysis: e.g. individual versus time-unlimited. Each category or group was given a "box-score"; the box-scores being the number of studies in which the treatment was either significantly better or significantly worse. If no statistical significance either way was apparent, a "tie-score" was attached to that study. The box-scores included 113 studies. Luborsky and his co-authors made use of a quote from Lewis Carroll's "Alice in Wonderland" in the title of their review which was designed succinctly to show what the general conclusion should be for those who read it: "Everyone has won, and all must have prizes....." Most of the studies quoted did report changes when compared with the controls. However, the by then familiar conclusion also emerged, namely

that such change as was reported, was uniformly observed across boxes: no single form of therapy was shown to be superior to any other. Another familiar comment was also reiterated, i.e. that with the exceptions of behaviour therapy and client-centred therapy, there was not enough good research adequately to compare or distinguish among the various "schools" of psychotherapy. This study itself has not remained immune from criticism, but it is nevertheless considered to be a highly sophisticated piece of work and the paper's enigmatic sub-title has reverberated around the outcome evaluation literature including the field of alcoholism evaluation research.

One of the concerns of psychotherapy research which has not been incorporated in the Alcoholism field, to its detriment, is the issue of therapist process variables, i.e. what clinicians in the field actually do in the presence of their clients, as opposed to what is supposed to occur in accord with some theoretical diktat. The work reviewed by Truax and Mitchell on therapist interpersonal skills would appear to have direct application to the Alcoholism field. Although no such work emerged in the Alcoholism field during the period being considered in this section, Davies (1979 and 1981) has reported on therapist client interaction from the perspective of discrepant expectations in an extremely enlightening way. However this work is well beyond the point in time under consideration in this section. Conversely an issue prominent in the Alcoholism literature but of little concern to psychotherapy was that of in-patient vs out-patient

care.

Throughout the latter part of the 1960s and the 1970s much theoretical and clinical debate was engaged in concerning the Nature and Treatment of alcoholism. Methodological expertise had developed at least on a theoretical plane. The major impediment to conducting outcome evaluation studies then and indeed now was that programme provision for such research endeavours in the everyday world of treatment centres was at best marginal.

By 1970 experimental and clinical psychologists working in the alcoholism field had in the main ceased to be two separate kinds of creature. Psychiatrists working in the alcoholism field had also come to accept the need for an empirical approach to their work. Traditional disease notions no longer held the centre of the stage, at least in research and specialist treatment settings. By the early 1970s numerous reports of controlled drinking had entered the literature and the first controlled drinking programme had been established (Lovibond and Caddy 1970). Multidimensional models of Alcoholism had been proposed to replace the old disease model. Patient treatment matching had been proposed (Pattison et al 1969: 1968). The reliability of abstinence as a measure of adjustment in other areas of functioning was found to be less than acceptable, hence the requirement for multiple outcome criteria (Pattison, op. cit.). Crucial concepts necessary for the maintenance of a rational acceptance of the traditional disease model of alcoholism were undermined, for example loss of control (Merry 1966). There

were by 1970 innumerable contradictions of the long cherished concept of irreversibility published in the research literature (Heather and Robertson 1981). It had been demonstrated that to varying degrees treatment worked, but not how it worked.

Throughout the decades leading up to the 1970s the increasing presence and influence of experimental and empirically orientated clinical psychologists was in evidence. As already noted psychiatry became empirically minded by the 1970s: crude behaviourism had gone through a number of transformations until a social learning theory emerged which was better equipped to give a more rounded or holistic perspective on behaviour. These developments owed much to the work of Albert Bandura (1969 and 1977).

I will conclude this section with the review by Baekeland, which illustrates by its structure as well as by its conclusions where alcoholism treatment "was at" at the close of the 1960s and the beginning of the 1970s. The review was published in 1977, a little beyond my demarcation point. However, most of the work contained therein was carried out between 1950 and 1970. Indeed, only 14% of the references are dated after 1971, and only 9% after 1972. Baekeland starts by telling us that the English language literature of the 20 year study period under consideration in his work, was "nihilistic" and that he wants to consider both in and out-patient studies. He then concludes his introduction with the following statement. . . . "The reader unacquainted with alcoholics may be surprised to discover that a high percentage

of alcoholics receive substantial and lasting benefit from treatment and it may astonish him even more to be told that therapeutic outcome seems pretty much the same regardless of the kind of treatment, but depends to a much larger extent on the kind of patient being treated". The review starts with a brief discussion of general topics, Viz:-

1. Treatment goals.
2. Duration of treatment.
3. Spontaneous improvement, or what happens in the case of the untreated alcoholic.

It is at this point that Baekeland divides the review into two parts in order to consider in and out-patient studies separately. Following this, he reviews the literature on Alcoholics Anonymous. Distinct sections address behaviour therapy and treatment which is drug-based. In his introduction, Baekeland had said that the early part of his study-period (1950s) had seen the introduction of Disulfiram and Citrated Calcium Carbamide (alcohol antagonists), then a little later tranquillisers and anti-depressants became available. He describes group-therapy as increasing in prominence over this period and comments that individual psychotherapy and Alcoholics Anonymous had been the mainstay of programmes since the Second World War. With regard to treatment goals he concludes that these should subsume all major psycho-social problems, and not simply problem drinking. As far as spontaneous improvement was concerned, Baekeland cites studies from the 1960s which reported spontaneous improvement; he says about this "there is a small but definite

chance that he (the drinker) will improve or recover due to non-medical influences". Cahalan in 1970, had shown on the basis of survey evidence that "drinking problems taper off after the age of fifty, especially in high Socio-Economic Status subjects". Cahalan concluded, "It thus appears that, depending on the patients' personal and social assets, there is a 2 to 15% chance of spontaneous improvement rate among alcoholics who do not receive formal treatment."

(Cahalan 1970).

In-patient treatment shows improvement figures of 41.5% or 30% if corrected for spontaneous remission. Baekeland comments that it is "either way very substantial" outcome for such a difficult condition. The question is raised - what should be applauded, the programme or the patient? Socio-Economic Status is strongly associated with a positive outcome, but so is intensive treatment. However he also comments that it would seem that those who drop out are the very ones who would benefit most from longer hospitalisation. In-patient psychotherapy is considered ineffective unless supplemented by follow-up treatment. Generally, in-patient treatment outwith the use of the hospital as a hostel does not earn its keep.

In the patient treatment section, Baekeland prefaces his scrutiny of out-patient services by reporting that from 1960 on there was an expansion in specialised out-patient treatment programmes and that they had improvement rates of 51% compared with 29% for general psychiatric clinics. He also comments that the negative bias of medical personnel

against alcoholics is well known. Patient drop out is seen as a much bigger problem for out-patient than inpatient clinics. Taking account of sample attrition, mean reported outcome for 18 clinics was 41.6 per cent (SD=15.3) improvement rate. This drops to 36% if 5% spontaneous remission is assumed. This is said by Baekeland to be quite respectable. With regard to patient vs treatment as outcome prediction, various demographic and psychosocial factors are involved, most importantly Socio-Economic Status and social stability. Baekeland concludes "that it would seem that the nature of the patient is much more important than that of the treatment used."

In the section of his review entitled "The Relative Value of Different Treatments", Baekeland again castigates psychoanalytically orientated individual psychotherapy with Alcoholics, noting the emergence of Couple Therapy as an "interesting and promising" approach. He concludes that "it seems that multidisciplinary treatment is more effective than individual psychotherapy and that in some cases active involvement of spouses in treatment is worthwhile." It is concluded at the end of the section on out-patient treatment that skid row alcoholics, "if carefully selected, can benefit from compulsory treatment if the penalty for dropping out is a stiff one."

Baekeland explains his rationale for having a separate section for Behaviour Therapy. It is "not because there is any compelling evidence that they are more effective than other kinds of psychotherapy, but simply because the

reader may be less familiar with their details." On the face of it this is a rather extraordinary statement to be made by a reviewer in 1977 when a vast behavioural literature was to hand. This section is quite meagre, understandably so given that most of the studies reviewed date from the early 1960s. Behavioural techniques are surveyed under three headings:

1. Aversive conditioning.
2. Systematic desensitisation.
3. Operant conditioning.

The final section of Baekeland's review considers drug treatment. In the conclusion Baekeland says: "It appears that multifactorial outcome measures are superior to abstinence alone as a criterion of success and that a six month follow up interval is the absolute minimum acceptable."

Importantly he comments, "We were repeatedly impressed with the dominant role played by patients rather than treatment factors, both in persistence in treatment and eventual outcome." Thus in in-patient studies good-prognosis patients (higher economic status and social stability) had improvement rates from 32.4% to 68% while poor-prognosis patients, largely skid row alcoholics, had rates ranging from 0 to 18%. No differences were found among different treatment regimes. "Out-patient clinics had higher improvement rates (41%) than did in-patient programmes despite their higher drop out rates (36.9% vs 17%)." The best outcomes again were observed amongst those with the highest Socio-Economic Status and social stability, whilst the worst outcomes were with skid row patients - "the evidence favours multidisciplinary

approaches, involvement of spouse in treatment, and forced treatment of selected skid row alcoholics."

With regard to behavioural approaches Baekeland says "if one takes into account that they are usually tried on highly selected volunteers, they seem to give about the same results as other treatment measures." This review is concluded with the sentence: "Patients who do well on drugs, psychotherapy or rehabilitation programmes seem to have different characteristics, and success rates go up with the number of treatment options given to the patients." One should not expect answers as opposed to further research questions from social research. Baekeland's paper, however, does deliver up some sort of status report on Alcoholism treatment at the beginning of the 1970s.

Prominently argued by evidence summarised in the conclusion is that treatment does work to varying degrees with even poor-prognosis patients. However the evidence is not well derived in most cases. The methodological problems persist. This conclusion that the major factor in good outcome is good patients in the sense that they are not too damaged socially and retain adequate social supports, is well taken. One is reminded of the conclusions of psychotherapy outcome research, namely: What treatment is called for? For which sort of patient? As things stand no one method has proved successful beyond any other.

CONCLUSION

Treatment provisions had not, by 1970, become elaborated generally, such that client matching was seriously attempted across the majority of treatment provision; although it had been an issue which was discussed in the literature from as far back as 1941 (Bowman and Jellinek 1941). The structure of Baekeland's long review illustrates by its differentiated format, the increasing complexity of outcome studies in the alcoholism field. Account is taken of in-patient and out-patient services, various treatment methods including behavioural techniques and even no treatment controls in the discussion of spontaneous remission. The review takes account of wider debates in the field and reflects the development of the multidimensional model and the corresponding presumption of multiple outcome criteria. Although a few controlled drinking studies were reviewed, these were treated as special experimental cases, and the predominant goal of treatment observed across the studies reviewed was total abstinence. Overall, by early 1970 the Alcoholism field was experiencing a good deal of confusion, but not disappointment. Services were still developing, and a new occupation of alcoholism counsellor was coming into being in the United Kingdom. This induced a sense of optimism. By 1970 the old stigma which had attached to "alcoholism" was fading.

CHAPTER 1 Part 2

A CHANGE OF MOOD

The period covered by this section is from the early 1970s to the early 1980s, although the middle of the 1970s will initially be considered as a period when a mass of important and influential research was being produced. In particular it will address five seminal studies:

1. The influential treatment review done by C.D. Emerick, in 1974 and 1975.
2. The report published by Barry Tuckfeld et al in 1976 and entitled, "Changes in Patterns of Alcohol Abuse Without the Aid of Formal Treatment".
3. The first Rand Report of 1976.
4. The Maudsley treatment and advice study of 1977 by Orford and Edwards.
5. The second Rand Report of 1980.

Treatment Review by C.D. Emerick

This review was published in two parts. The first part is entitled, "The Use and Interrelationship of Outcome Criteria and Drinking Behaviour Following Treatment" (June 1974); the second, "The Relative Effectiveness of Treatment versus No-Treatment" (January 1975). Since their publication these two papers by Emerick (1974, 1975) have been the most frequently cited reviews in the literature. Emerick's review covers 271 studies published between 1952 and 1971. Commenting upon the publication rate over the period being considered, he tells us in the introduction to his first paper that between 1952 and 1955, eight studies were published

annually, and that the rate increased to 20 per year for the period 1967 to 1970. Emerick additionally tells us that there was a "staggering array of treatment approaches" that had been evaluated and that he was obliged to make use of a broad sociological definition of alcoholism so that the variations in definition to be found in the studies could all be subsumed within his review. In the discussion of the Baekeland review it was noted that "confusion" appeared to be the most apt description of alcoholism treatment at the beginning of the nineteen seventies. It is the opinion of this author that Emerick's work marked a transition point; a move away from this "confusion".

His first paper addressed three questions: the outcome criteria used; the interrelationships among these criteria; and the impact felt to have been made on the drinking behaviours of the subjects under study. Because of the fact that the studies reviewed had made use of a wide variety of disparate measures as outcome criteria, Emerick clustered together those that had "logically belonged together", ending with 19 groups.

Drinking behaviour was the most frequently used criterion, and in some studies the only one. Emerick cited five studies in an attempt to show that abstinence per se does not necessarily correlate 100% with other standard criteria, and even that deterioration had been observed after abstinence had been achieved. In this same regard, however, he did comment that "drinking relates positively, though not perfectly, with many other indices, and it can serve as an

important indicator-criterion in alcoholism research". The relationships between drinking and other criteria in the studies covered by Emerick were analysed. Drinking was found to be a good predictor of improvement in most significant areas of functioning, social, vocational and psychological. As a consequence of this finding of the comparative analysis between drinking versus other criteria, other criteria were dropped from subsequent analyses in the review. Many studies in any case used drinking behaviour as the sole outcome measure.

Overall outcome rates were computed: results were calculated both for individuals across studies, and also rates for specific study-samples. Nine outcome categories were identified from :-

1. Totally abstinent throughout the follow-up period, through
5. Much, or somewhat improved, to, finally,
9. Worse.

The result of the analysis of overall drinking outcomes was as follows:-

Abstinent	31.9%
Somewhat improved	17.9%
Controlled	5.2%
Much, or somewhat improved	29.4%
Abstinent/controlled	43.4%
Worse	10.4%
Total improved	66.0%
Total unimproved	34.0%

One third were abstinent during follow-up, and one twentieth were, or had become controlled drinkers. Two thirds were "improved to some extent". Here again we have an instance of the "rule of one-third" referred to earlier.

Emerick provided an interesting actuarial table of outcomes from the data listed above (reported with median and standard deviation). He suggested that the mean and median estimates provided "a context for judging how usual or unusual the results of any one study are as guidelines for evaluating the commonness of results." If one standard deviation above or below the mean is taken as a point beyond which an estimate is judged atypical, the following indicators or benchmarks are formed. Abstinence rates have to be below 10.5% or above 53.3% to be unusual. Abstinent or controlled below 19.8% or above 67%; much improved below 4.7% or above 26.3%; total improved below 47.8% or above 84.2%; total unimproved below 15.8% or above 52.2% and, finally, deteriorated below 0.2% or above 20.6%.

Emerick notes in the conclusion to his first paper that, "treatment leaves some alcoholics abstinent, some improved though not abstinent, others unchanged, and yet others worse. The vast majority, (about two thirds) are improved or abstinent, indicating that once an alcoholic decides to do something about his drinking and accepts help, he stands a good chance of improving". Emerick concludes by saying that his review had raised two further points. These were dealt with in the second paper and are as follows :-

1. Whether an alcoholic might increase his chances of

improvement by having one form of treatment rather than some other.

2. Whether the likelihood of improvement would be just as strong, or stronger, with no formal treatment.

In his second paper, Emerick (1975) deals with the effectiveness of various treatment regimes and the issue of treatment versus no-treatment. He enlarged his sample of studies by 126 by extending his review from 1952 to 1973. The total pool of studies then becomes 397, seven of which were single case studies and were not analysed. Overall, however, the amount of work which was overviewed was very substantial. Part of this analysis involved addressing the two questions posed at the end of his first paper.

Concerning those studies (72 in number) with two or more randomly assigned conditions, or matched on crucial characteristics known to correlate with outcome, Emerick searched for differences. In addition he cites evidence for outcome data that represented behaviour stabilisation after a six-month follow-up. In other words he restricted his analysis to those studies/cases having differences extant at least six months after treatment. He found that 31 studies evinced no difference among differing treatment groups. On the other hand, 41 studies did show differences: all except five of these studies had been evaluated at minimum time-periods, and the treatment involved had lasted for at least one year in one of the groups looked at. So, the basis for comparison either could be regarded as unfair, or the differing outcomes as artificial.

Emerick comments, "Thus, in the vast majority of cases, significant results were short-term, due to chance, or not directly related to life-adjustment". He then makes the striking point that, in the few instances where long-term differences were found, such results were to be explained not by virtue of one treatment being decided successful, but rather by seeing the comparison treatment-group having had harm inflicted in some way such that improvement was retarded. In the five studies cited where progress was retarded, this was usually traceable to the clients having been disappointed at being assigned to a control condition such as being discharged from a programme early or denied the treatment of their choice. Because the studies concerned did not control for iatrogenesis, no conclusion could be reached vis-a-vis the relative success among the more favourable outcomes. This, of course, does not mean that these five positive outcome studies did not contain an "effective ingredient". Emerick suggests that "the weight of present evidence is overwhelmingly against technique variables as being powerful determinants of long-term outcome".

These results led Emerick to suggest that the search for better treatment methods ought to give way to a strategy simply of getting alcoholics into treatment, any treatment, since "all get prizes". He also suggests that the therapy of the client's choice might be indicated. With reference to treatment versus no-treatment, Emerick makes the interesting point that no treatment improvements and minimal treatment improvements indicate only the absence of formal treatment

and, not necessarily, a concomitant lack of therapeutic experience. It is for this reason that he avoids using the more usual term "spontaneous remission". He feels that this term suggests "that changes occurring outwith therapy arise naturally out of life-circumstances and conditions, whilst changes within therapy follow a different process".

In computing the outcome comparison-rates, only two categories from the nine category system described in the first of Emerick's papers were used. These were, abstinent, and total improvement. These were felt by him to be the only two categories logically able to subsume the minimally treated groups. Again, follow-up was at least six months after treatment. The results of this analysis showed that from the non-treated group, 12.9% were abstinent whereas 20.6% of the minimally treated group had become abstinent (although there was a much wider range for the non-treated group; s.d. 16.9 as opposed to 3.6). For the category of "total improvement", the non-treatment group scored 40.8, and the minimal treatment group 42.6. Emerick advises caution in studying these figures because of the varying sorts of data derived from a small number of studies, and because no control was exercised over patient characteristics when the comparison was made between non-treatment and minimal treatment groups. The conclusions reached however, are still forceful:

- (1) "Many alcoholics can drink less or stop altogether with no treatment or with only minimal treatment", and
- (2) "Untreated alcoholics change as much as those receiving minimal treatment".

Having established that for practical purposes, no treatment and minimal treatment groups can be subsumed within a single category, the major comparison is then made between this composite group and the more-than-minimal treatment group.

There were no differences between the two groups when the group comparison was analysed. The figures were 15.9% for no treatment, and 24.5% for treatment on the abstinence criterion (5 groups versus 45 groups). However, when the total improvement score was computed for the comparison groups, and for individuals, a difference was found. "Both per group, and per individual total improvement rates were positively and practically related to treatment amount". This finding accounted for 36% of the variance in group rates. Again, the same caveats apply as in the case of the first analysis. Emerick concludes, "Nevertheless, this finding suggests that alcoholics are, in a practical sense, as likely to stop drinking completely for six months or longer when they receive no, or only minimal treatment, as when they receive more-than-minimal treatment".

The additional finding also applies, viz. "Treatment seems to increase an alcoholic's chances of at least reducing the scale of his/her problem". Perhaps commonsense explanations are appropriate here concerning the learning of new social skills in treatment, either as a direct component of treatment, or in a social skills type of group (though it has to be said that this latter kind of group hardly existed in any numbers during the study-period covered by

Emerick.)

Emerick has established a significant spontaneous remission rate and he contradicts Eysenck and Beech (1971) who claim that alcoholics do not experience spontaneous remission. Having said this, such a comparison is complicated somewhat by the inclusion of the "minimal treatment" situation which, in operational terms vis-a-vis this review, means less than 5 out-patient visits or, two weeks in-patient treatment. Nevertheless, in comparison with the no-treatment group, no difference was observed. Therefore, Emerick was not risking much when he compared the minimal/no-treatment total improvement rate of 41.9% with the median improvement rate of 30% (Begin 1971), for a similar group of neurotics receiving only minimal treatment.

Having essentially demonstrated that treatment variables count for next to nothing, Emerick makes the curious suggestion that treatment agencies should be "heartened to see some indication that treatment is effective". No doubt he means that most treatment is not harmful and thus to some extent successful in not being an obstacle to spontaneous remission and that treatment-settings possibly offer a domain wherein patients can learn coping skills from one another. Neither of these papers weighed in with the customary methodological criticisms one so frequently finds in the literature, and it is clear, by virtue of the fact that the second paper was possible, that methodologically, studies were becoming more sophisticated.

As was mentioned earlier, the Emerick review of 384

studies between 1952 and 1973 is the most intensive and influential to date, and despite the author's final comment about the "heartening" nature of his data for treatment personnel, the review nevertheless reveals a very bleak state of affairs in the alcoholism treatment business. This last conclusion was not lost on the alcoholism treatment and research community; across a wide range of studies and follow-ups it was found that approximately one-third of patients were abstinent and two-thirds usually improved. This is a slightly optimistic modification of the "rule of one third", and as such is good news as far as it goes. The bad news of course is that treatment appears to have little to do with this improvement. Of the 36 studies (matched and randomly assigned), only five reported any difference between treated and non-treated; and those were considered to be unfair comparisons. Treatment scores a strong point in that five studies reported drinking outcomes of minimally treated patients that were more improved than those who had received no treatment.

The 1976 Tuckfeld Study

The next main research production to be examined is the last report for The National Institute on Alcohol Abuse and Alcoholism (Tuckfeld 1976). This is not so much a treatment outcome study as an anti-treatment outcome study. It is extensive and of interest because its results have had an important bearing upon the views on treatment that were forming among professionals during the latter half of the 1970s. It expounds new views to the alcohol research

community about the important phenomenon of spontaneous remission insofar as it goes one stage beyond the hitherto usual "head counting" and attempts to describe in considerable detail, what actually happened to people who recovered outwith the treatment domain, such that the traditional concept of spontaneous remission, with its adventitious connotations is presented as inadequate. Furthermore, this was all achieved by the use of what were and still are for the alcoholism field, novel methodological practices: the use of both qualitative and quantitative methods.

Tuckfeld states in the preface to his report that the primary objectives of this study were :-

1. To confirm or to disconfirm the actuality of beneficial changes amongst individuals who have alleviated problems with alcohol use without the aid of formal treatment.
2. In the case where the above is confirmed, to explore the factors and processes associated with these changes.

He stated that the "research effort was oriented to 'discovery' rather than to make statistical inferences or for theory verification". The methodology centred around, "life-history abstracts based on a standardised format; qualitative information was summarised". The majority of the individuals studied were contacted initially through a mass-media campaign. Only those people who reported being trouble-free for at least one year and who had not had any formal treatment for their pre-existing drinking problem were considered for the study. Initial screening was carried out by questionnaire, then an assessment interview was conducted to

determine caseness for intensive analysis. Issues of internal consistency and comparability of questionnaire and assessment interview where the life-history abstracts were completed, were undertaken by the research team. A total of 51 cases were found, their average age being 48.6 years. Only seven were black, and only 14 were women. The average length of time since their problem resolution was 6.4 years. Of the 51 cases comprising the study-population, 11 reported engaging in trouble-free drinking. All had reported severe problems with alcohol previously. Apart from the 11 trouble-free drinkers, the remaining 40 were all abstinent. Because of the methods through which the study group was assembled, there were no claims made for representativeness or for generalisation of the study outcomes. As Tuckfeld explained, the study was exploratory so that these issues are not of crucial importance.

The study findings were :-

1. Resolution of alcohol problems outwith formal treatment does occur.
2. This is effective for some people.
3. The process and associated factors are amenable to empirical investigation.

Tuckfeld comments, "In general, it was found that several types of problem drinker, including 'alcoholic' persons, sustained a state of problem resolution without the aid of formal treatment". He noted that one prominent reason for people resisting treatment was a refusal to be labelled 'alcoholic'. The reasons given for individual decisions to

cease problem drinking were numerous, but as one might expect most were related to an unpleasant experience like having to endure extreme humiliation or health problems. An important exception was that of a religious conversion experience. Information concerning alcohol abuse and previous experience of being self-controlled (i.e. giving up smoking etc.) were also numbered among the various reasons put forward.

Tuckfeld's research found that the resolution of problem drinking was not merely an issue of making up one's mind to stop or to control the use (misuse) of alcohol. Such commitment to change one's drinking habits was insufficient of itself to sustain movement in the direction of problem resolution. Social conditions were important "maintenance factors", involving social support from one's family and friends who offered encouragement and a propitious leisure environment. These environments are described in Tuckfeld's report as being "relatively stable social and economic support-systems".

With regard to the concept of spontaneous remission, Tuckfeld comments that "few, if any cases in this study could be characterised as 'spontaneous'; developing without any external influence". To illustrate this point, a process model of the transition process has been constructed (see diagram page 48?). This model entails the interrelationship of social and psychological variables with crucial stages through which the individual may pass.

The main components of this process are as follows:

1. Recognition
2. Disengagement.
3. Interim changes in alcohol-related behaviour.
4. (and if successful), Sustained change.

This process, although formalised in the shape of the model presented, when viewed from the qualitative stance of the life-history abstracts (included in the report), has very much the ring of commonsense about it.

One of the hypotheses suggested by the research which is in contradistinction to, and in conflict with traditional views of alcoholism is suggested by Tuckfeld, viz. "The acceptance of the 'alcoholic' label is not a crucial factor to persons who resolve their alcohol problems without the aid of treatment." By implication of course, this leads to a generalisation that treatment populations writ large may be susceptible to the same rationale. Indeed this view was already accepted by the WHO expert committee on alcoholism at the time that Tuckfeld published the research. Such a view was not, and to some extent still is not generally diffused amongst treatment personnel. It is worth lingering a while on the subject of Tuckfeld's study for a number of reasons. Although I have conceived it as being in a sense representative of the spirit of the decade, and as having a significant influence upon subsequent research in this field, I nevertheless feel that perhaps too much attention has been paid to the overall results of the study: namely, that people recover from drinking problems without the assistance of

professional treatment personnel. This may deflect attention away from the interesting process model developed by Tuckfield to account for his results. Concomitantly, it also diminishes the significance of the interesting methodology employed in the course of the study. The life-history method is of course not new and there are notable examples of its use in the criminology literature. Moreover, in the alcoholism field Robert Strauss (1946 and 1948) has provided a detailed account of one single case . Given the pseudonym "Frank Moore", this person was termed by Strauss "an itinerant inebriate". Strauss apparently first encountered this man in 1945 and contact was maintained for a period of 27 years. "Frank Moore" provided Strauss with an extremely co-operative subject, and detailed accounts of his biography were the result. (Strauss 1973, 1974).

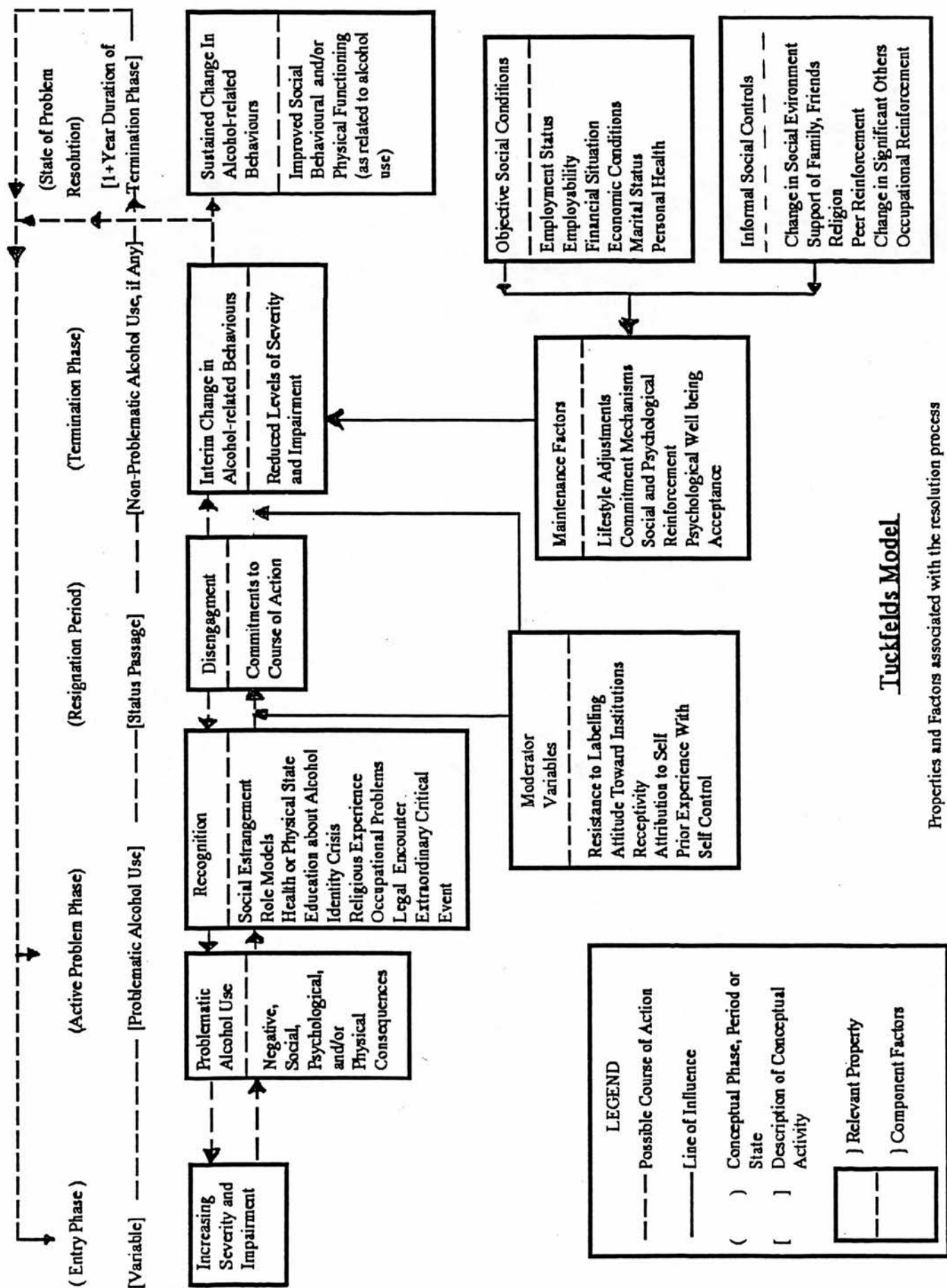
However, Tuckfield's study does far more than simply recount a single biography. By embedding the qualitative data matrix, he transforms the life-history so that reasonable comparisons of events can be catalogued across his study-group in a standardised format suitable for data-analysis; and out of this data-analysis, important albeit general, inferences can be made. Information is gathered initially in an informal manner during discussions with clients. The data collected are then sifted for inclusion in one of six relevant categories as follows :-

1. General background.
2. Drinking history.
3. Life immediately prior to resolution.

4. Apparent precursors of problem resolution.
5. Post-resolution life-style changes.
6. Putative maintenance factors; post-resolution.

The above strategy allowed Tuckfeld to penetrate further than most (certainly within the alcoholism field), into the recovery process. What are predominantly at issue here from a methodological perspective, are perennial problems in social science research relating to the failure of empiricism comprehensively to capture the ebb and flow of social life. By strict empirical canons, many objections can be raised about the validity of this kind of approach; moreover, the argument then becomes a philosophical one concerning not the validity of empiricism per se, but rather the positivistic kinds of theory from which empirical methods originally are derived. These issues will resurface in this thesis. At this point I wish merely to acknowledge that contentious methodological and philosophical issues are implicated. For the present, I simply want to argue that Tuckfeld's method is both genuine and effective in the production of extremely germane data relating to the recovery process. To continue with a comment from the study, the various drinking careers showed a wide range of variability prior to the phase in the model designated "active problem phase". Three general patterns were however discerned.

1. Habit that may/may not be related to personal problems.
2. Social dependence, (following the example of others).
3. Social/Psychological stress.



Tuckfelds Model

Properties and Factors associated with the resolution process

As can be seen from a visual scrutiny of the model, it is given temporal direction insofar as it follows a phase sequencing. There are four main time phases to the process, and a fifth outcome-state:

1. Entry. (As noted, Tuckfeld received many reports concerning drinking careers prior to "entry").
2. Active drinking problem phase.
3. Resignation period. ("Status Passage" from active problems to non-active problems).
4. Termination phase (Absence of drinking problem).
5. State of problem-resolution.

The interesting data-based components are displayed in the boxes in the diagram. The four crucial properties of the "resolution process" model also appear in boxes in the diagram, they are, (excluding entry and active problem phases) as follows:-

1. Recognition.
2. Disengagement.
3. Interim phase in alcohol behaviour.
4. Sustained change in alcohol related behaviour.

Immediately below the dotted line in the diagram are more specific factors that relate to the aforementioned major properties: i.e. the major properties of recognition (that one has an alcohol problem) are specifically determined by the various states of affairs listed in the box under the dotted line. Examples are social estrangement, religious experience, occupational problems, legal problems, etc. The model becomes more complex with the addition of moderator variables and

maintenance factors which are impacted upon by objective social conditions and informal social controls. It might be commented here that the complexity spoken of is in some sense artificial; there is no difficulty at the conceptual level, it is merely that, at its most elementary, social behaviour is multifactorial and very difficult to schematise.

The most important aspect of this model for the purposes of this chapter concerns the relationship between the moderator variables with the process of disengagement, and the maintenance factors with resolution.

As one should be able to discern from the diagram, the moderator variables are in fact self-report data of a new sort and their significance in the overall process is crucial. Persons making a verbal commitment to change have to transform this into a behavioural fact in order to pass onto the next stage of disengagement. The several factors which in this study appear to influence an individual's resolution to go on to "get it done" are as I have enumerated below:-

1. Resistance to "labelling".
2. Attribution of responsibility to self.
3. Prior experience of self-control
4. Resistance to institutional labelling.

These moderator variables, claims Tuckfeld, "are maybe particularly salient". They help us to understand why it is that some persons pass from recognition to termination in terms of the model. With respect to generalisation to treatment populations, Tuckfeld thinks that there are good reasons to believe that "these variables might be

useful in the identification of sub-groups within treatment programs for personalized treatment plans and more precise evaluation of efforts."

The next crucial aspect of the model concerns Termination and Maintenance factors. My own intuition about these matters is that treatment programmes generally are reasonably effective at getting people to the termination phase: getting them off alcohol or achieving control. The problem is to keep them off, or to keep them in control of their drinking habits. That is to say, to reach a state of Problem Resolution in terms of the model.

I would therefore see these sections of the model concerning termination and maintenance factors, as being the most important of all of the model's components for what it can tell us about alcohol dependence recovery and its evaluation. It becomes apparent that the moderator variables operative during the resignation phase (status passage) of the process are characterised, at least in their expression if not in their origin, by cognition as befits essentially cognitive tasks immanent in decision-making. By comparison, the maintenance factors are predominantly cognitive in neither expression nor origin; they are very definitely social. In other words they are variables that are manifestly external to the individual. Tuckfeld comments, "in line with that view, were the reports from numerous respondents of the value of reinforcements from family and friends." Also many respondents reported significant alterations in social and leisure activities. Some respondents sought out situations

that would entail informal controls. Here, Tuckfeld introduces the idea of "life-style readjustment" to describe the effect of those maintenance factors concerned with re-socialisation and/or novel leisure pursuits. He says that they are considered to be "necessary as re-inforcement from significant others". At this termination phase, acceptance of one's situation was often accompanied by increases in self-esteem as part of the larger process of "identity transformation". Another maintenance factor is "getting religion", this is described as a "commitment mechanism".

Education about alcohol problems is also seen as relevant. It is worth mentioning here that Alcoholics Anonymous members argue that education is one of the most cure-potentiating factors of the Alcoholics Anonymous programme. The Alcoholics Anonymous view of alcoholism enables alcoholics to make sense of their past lives.

Tuckfeld borrows the term "vocabulary of motives" from the sociologist C. Wright Mills to describe this phenomenon (Mills 1940). Social support and informal social controls did not of themselves produce maintenance factors, they were "often interactive with objective social conditions". Thus, being employed or otherwise financially secure was important. Some of Tuckfeld's respondents who were not particularly well-off or employed, reported that they had friends and family who might help out financially in a crisis, so an essential sense of security was enjoyed. Again there is ample evidence from the literature on stress and social support to confirm the importance of this "sense of security".

Commenting on this overall control process, Tuckfeld considered that no single factor was in itself a sufficient condition. All were necessary in an interactive process. "While particular emphasis was given to a variety of moderator variables, the process was depicted as having equal emphasis on social supports and informal social controls that interact to facilitate maintenance factors". The important observation was made that different factors are relevant to different phases.

Tuckfeld compares his model to a similar study of treatment processes (Bigus 1974). The main differences between the two processes concerned the issue of moderator variables, just as would be predicted in fact by the Tuckfeld model. These do enable one to explain why people experiencing similar events, "may or may not utilise different treatment resources". What is highlighted here is a new set of client variables which may be implicated in the issue of attrition. Furthermore, although to this author's knowledge no-one has ever tried to look for these, they may yet describe a subgrouping of treatment drop-outs who ultimately succeed.

In other respects there are close similarities between the treatment factors reported by Bigus's Institutional controls and the "informal social controls" of the Tuckfeld study, as having a bearing on the Maintenance Factors. Baekeland had raised this matter (as was mentioned previously in this discussion) as to who or what should be congratulated: "Should it be the treatment or should it be the patient? . . ." asks Baekeland. On the basis of the



Tuckfeld study, one would in all probability urge that any felicitations be afforded to patients, their friends and their families. This is not a trivial point for treatment evaluation research. The issue of "natural healing factors" is one that has come to the fore during the decade under consideration in this section. (Vaillant 1980; Orford and Edwards 1977).

I wish to make some final comments before leaving this study by Tuckfeld et al. I have covered his work at some length since its concerns are very much in keeping with the main thrust of this thesis; also the methodology that Tuckfeld uses is or should be seen as paradigmatic for research into alcoholism recovery. In addition, from the very instructive model that Tuckfeld presents, it becomes clear that the over simple notion of "spontaneous remission" is untenable. Tuckfeld suggests the replacement of this phrase by one of his own: viz "The resolution process".

Another conclusion of this study that has not been taken up by researchers is the one that, "some persons are ideologically, not just psychologically resistant to treatment." The overall conclusion here is that "the status passage from a cognitive state to a social action must be supported by constructive social conditions that encourage behavioural change."

The methodology of the Tuckfeld study is noteworthy since it is in marked contrast to most others done previously or subsequently. Tuckfeld and his team have taken a somewhat obsolescent sociological research instrument and, with the aid

of new technologies have galvanised it to create a very effective new tool. To the best of my knowledge, the life-history technique, as used by Tuckfeld, is a completely novel way of generating research data: I can find no comparable research strategy in alcoholism studies to which it could be realistically compared. Nevertheless, the life-history abstract method has a noble pedigree, in the Department of Sociology at the University of Chicago. The "Ecological Approach" of the "Chicago School", adapted participant observation as a research tool in the 1920s; the philosophical and logical models that underpin the method are inappropriate for the study of social life such that the underlying *modus operandi* had to be altered. Tuckfeld did in fact alter the original format or structure of the life-history method and, in my opinion, by doing so set a milestone in alcoholism research methodology.

As a method, the life-history abstract technique owed much to the journalistic skills of Robert Park; a founder and pioneer of the "Chicago School". The method developed out of a participant-observation technique designed initially to meet the needs of more fine-grained or refined information-gathering, because, in its original guise, it was designed to collect information unique (that is to say biographically unique) to individuals under study. It was viewed as unscientific insofar as the biography of an individual is unique and unrepeatable in detail and sequencing. In this sense then, it was seen more as a way of evincing information that would pose rather than answer questions. The "answering"

putatively would occur through the application of more "scientific" (replicable) methods.

Life-histories formed the basis of much important work done by the Chicago School. Its most eminent proponent was perhaps Clifford Shaw (1930). The method was described by Burgess (1945) as a "Social microscope". This indeed was how Tuckfeld used it but without the disadvantages that originally beset it and which were exorcised by Tuckfeld after he had studied the criticisms of it made back in the 1930s. In this last respect Stouffer, twenty years after these criticisms were made, was able to demonstrate that attitude scales were just as good as the life-history method, and cheaper to administer (Stouffer 1950). Contrary to Stouffer, my feeling is that life-histories do much more than merely allow one tediously to derive respondents' attitudes; the texts of such histories record events and series of events which can be categorised along various dimensions independently of concurrent attitudes.

Eastthorpe (1974), in his discussion of the development of the life-history method, comments upon the fact that life-histories describe unique events which are nonetheless prescient of the possibility of a survey of a concatenation of life-histories. "A collection of life-histories . . . could be used to find regularities, but there have been no attempts to do this". This in fact is what Tuckfeld has done in his study. He has infused the old life-history method with the vigour and vitality of the new technologies, with modern approaches to data-analysis such

that a greatly increased potential for generalisation is realisable. Moreover, as this is achieved there is no trade-off in the "microscopic detail", an abandonment of the journalistic approach notwithstanding. The life-history method of data-collection is, in my view, the ideal way to proceed in many areas of alcoholism research. Certainly it has provided data which should be of crucial concern to the study of alcoholism treatment-outcomes. I will return to Tuckfeld's study because the data generated have raised many vital issues which impinge upon and are enmeshed with the concept of Affect-Balance, which forms the subject matter of the next chapter of this thesis.

The First Rand Study

The next major treatment evaluation event of the decade concerns the first Rand study. This report is entitled "Alcoholism and Treatment" (Armor et al 1976). Overall, the Rand studies comprise three reports: two major reports (Armor et al 1976; Polich et al 1980) and also a component study (Ruggles et al 1975). This latter was a follow-up, at 18 months, of the 1976 report cohort, although it was actually published in advance of the main report.

The first Rand study generated considerable controversy and in the field of alcoholism research a degree of turbulence, not unlike the furore that followed the publication of the Davies paper fourteen years previously. However, the debate which followed the Rand report was far more public and acrimonious in its tenor.

In 1971 the United States Federal Government created the National Institute of Alcohol Abuse and Alcoholism (N.I.A.A.A.) as a major governmental response to the problems of alcoholism that were arousing national concern. It was claimed that the emergence of N.I.A.A.A. was the second major policy initiative of the century in the field of alcohol studies, the first being Prohibition. The N.I.A.A.A. immediately set about funding treatment agencies and the Rand corporation was contracted to undertake an evaluation of the federally funded agencies and treatment programmes. Programme evaluation was built into the programme-planning. The N.I.A.A.A. established an alcoholism treatment monitoring system which collated, uniformly, data from all of the

N.I.A.A.A. funded programmes.

It is manifest therefore, that the establishment of a uniformly structured data-collection system was an historical landmark in programme-evaluation which conferred immense advantages on the Rand researches since they had the benefit of uniform data-sets from which to work. This is in sharp contrast to all previous researchers, like Emerick who laboured hard in order to distil or infer conclusions from often quite widely divergent data-sets.

The N.I.A.A.A. alcoholism-treatment studies have generated a vast amount of demographic and clinical data, at intake and at six months. By the time of the study, there were data on some 30,000 clients who had been treated by 44 agencies. At the six-monthly follow-ups, there was usually only a contact-rate of 25% of those who had entered treatment. Moreover, a six-month period for follow-up can only point to short-term outcomes. A special 18 month follow-up was designed to correct for this sort of shortcoming. (The work was in fact carried out by Stanford University Research Institute (Ruggles et al 1975)). For this survey, a sample of eight Alcohol Treatment Centres' clients 18 months after intake was followed up. Two thirds of the sample completed the data forms.

A third data-base was available to the Rand researchers to use as a comparison group. This came from a N.I.A.A.A. series of surveys (between 1972-74) which had been undertaken by a polling organisation, Lewis Morris and Associates, who had sampled the opinions and drinking

behaviour of 6,000 respondents. This work had been commissioned to evaluate the impact of a national public education campaign. Similar data-collection instruments were used in these surveys to those used by the monitoring system, and the data-sets were all collected at the same time. The Rand researchers had access to data, the comparability and generalisability of which had never been seen before in the alcoholism research community. These data included both treated and untreated, as well as minimally treated individuals. Armor et al (1976) tell us the major goal of their report was to provide a broad evaluation of alcoholism treatment and its aetiological implications by investigating alcoholics in treatment as well as alcoholics and problem-drinkers not in treatment.

An evaluative model which guides the analysis for the study is derived from a literature review which comprises the first chapter of the report (The first Rand Report). It is an "input-output" model suitable for the testing of treatment effects and includes definitions of major categories of client and manner of treatment considered appropriate for each client. The model is then tested using the National Survey Data and their own alcohol treatment centre data.

The model has three factors:

- a. Client-input, the components of past care; symptomatology, drinking context and history, social background, social stability, psychological attributes and physical characteristics.
- b. Treatment inputs comprising treatment-setting techniques

and amount of treatment; therapist-characteristics and facility-characteristics.

Both clients and treatment inputs interact with each other, and both of these interact with the next factor:

c. Major outcomes for recovery, which are: drinking behaviours; behavioural impairment; social adjustment and relapse.

The authors tell us, "whilst the input-output model is not itself a causal theory or a remedy-theory for alcoholism, it does enable us to test some of the research questions that are generated from the aetiological and prognostic perspectives reviewed in earlier sections". The authors claim that their input-output model enabled them to go beyond a simple assessment of treatment success; that it facilitated an examination of treatment outcomes for "Alternative theories of alcoholism". This is perfectly true and is of course one of the reasons why the report generated such a heated debate. It is this element of interpretation which makes the Rand study results different, not the outcome data themselves which, as many commentaries have suggested had, to a significant extent, been reported in numerous previous studies over many years. This being the case, an inference about the scientific, or more correctly quasi-scientific, nature of alcoholism research can be made.

Like the Gerard and Saenger study, the Rand study proposed a three-stage category definition for remission. The outcome-criteria for remission "are based strictly upon drinking behaviours; social adjustment indicators such as

marital status, or employment, are excluded. The Rationale is a desire to keep the definition of remission conceptually close to the condition of alcoholism per se; a condition we view as a physical and psychological dependence on alcohol". These definitions are :-

- A. Abstained for six months.
- B. Abstained for one month.
- C. Normal Drinking.

All other patterns of consumption were categorised as non-remissions. Much of the heated debate that followed the Rand study concerned the use of the term "normal drinking" for the third category. "Normal drinking" in the study was defined as consumption of less than 3 fluid ounces of alcohol per day over the past month, unaccompanied by any symptom of dependence or of behavioural impairment. (3 fluid ounces of alcohol is roughly equivalent to three and one-half pints of beer). This estimate was "normative" in the sense that the population averages had been used from the survey data. The subsequent arguments often confused healthy, or desirable, with normative drinking. This contentious term, "normative" was substituted with the alternative phrase "non-problem drinking" in the 1980 report. The Rand researches demonstrated that clients entering the programmes were severely impaired as a result of their drinking; over three-quarters were "definitely alcoholic", drinking more than 12 fl. ounces of alcohol per day, (approx. 12-15 pints of beer or a bottle of whisky) or have had dependence symptoms. "They drank nine times more than the average", and had, "adverse

behavioural consequences at a rate of more than 12 times that for non-alcoholic persons". Half of them were unemployed and more than half were separated or divorced. Despite this profile some commentators tried to argue subsequently that the Rand subjects were not really alcoholic in the first place.

With regard to outcome, and despite poor prognostic indicators in terms of the model, treatment appeared to be successful, at least at first glance. At six months, 68% were in remission, 18% were long-term abstainers (6 months+), 38% had abstained for between 1 and 5 months and 12% were "normal" drinkers. (The average consumption for this group was below the threshold level, being about two drinks per day). By 18 months, little had changed. Relapse was defined as being in remission at three months and being a non-remission at eighteen months. No significant differences were found on comparing the various remission groups at six months and at 18 months. Long-term abstainers accounted for 17% at 18 months as opposed to 18% at six months. Short-term abstainers were 19% as opposed to 38% at six months and normal drinkers were 13% at 18 months and 12% at six months. However, these figures track groups of repeated remissions and not individuals. That is to say, it is not the case that the same individuals reported long-term abstentions at six, and at eighteen months. In fact only 10% reported being long-term abstainers at both points in time. The authors of the report conclude "the majority of improved clients are either drinking moderate amounts of alcohol, below what would be considered

alcoholic drinking, or engaging in alternating periods of drinking and abstention".

Overall, nearly 70% were in remission at 18 months. Despite considerable shifting between remission categories, nearly two-thirds remained in some remission grouping. The authors of the report stress the key finding of the relapse analysis, "that relapse rates for normal drinkers are no higher than those for longer-term abstainers; even when the analysis is confined to those who are definitely alcoholic at intake".

On the face of it these are optimistic results. However, when the comparison is made with data from untreated clients and account is also taken of type and amount of treatment as indicated by the input-output model, a familiar picture emerges:

A. Treated clients had only a slightly higher remission-rate than those individuals who had had only a single contact with the treatment programme. Subdividing the sample again, it was found that only those clients with the highest amount of treatment showed any advantage. Those with lower amounts of treatment had remission-rates only slightly above those who had received no treatment at all.

B. The remission-rate for the untreated sample was 50% as opposed to 70% for those receiving treatment; also, untreated respondents who attended Alcoholics Anonymous achieved this figure of 70%. The 20% seemingly accounted for by treatment is an extremely pessimistic comment upon the efficacy of formal treatment.

The analysis by programme type again reiterated a familiar result, viz. that there were no differences in remission-rates across programmes. The programmes varied between in and out-patient and half-way houses. A comparison was also made between differing therapeutic techniques like drugs, group and individual methods; still no differences were found. Echoing previous reviewers, the authors state, "It appears that the fact of treatment is more important than the specific type of treatment, with the important proviso that to produce a remission-rate exceeding that due to natural processes, the treatment must be given in sufficient amounts." These researchers make the point that because of the more experimental nature of their data, there exists the possibility that self-selection might explain the uniform rates of remission, but this hypothesis cannot be tested. However, they do remind us that the same uniformity has been observed in studies using random allocations.

Multi-dimensional models of alcoholism were tested by searching for client-by-treatment combinations. However, no significant interactions were found. The usual client characteristics regarding social and marital stability were found to relate to outcome but they were not mediated by any particular treatment-interaction. With Tuckfeld's work in mind, this finding hardly seems surprising, given the conventional nature of the treatment on offer.

An interesting theoretical point is offered by the Rand researchers prompted by these findings concerning the uniformity of remission-rates; it is that they tend to deny

the central aetiological and treatment focus on psychological factors. "Our data suggests once alcohol dependency or addiction is established, that non-psychologically oriented treatments work as well as any other method. In other words, recovery from alcohol dependency may depend on mechanisms quite unrelated to factors that led to excessive drinking in the first place."

An unfortunate finding by Ruggles (Ruggles et al 1975), at the 18 months follow-up, was that despite the rather high remission-rate, hardly any change occurred in marital status. Perhaps it is the case that 18 months is too short a period to look for reconciliation in marriage (the rate of broken marriages was found to be 40%). Unemployment standing at 50% prior to treatment did however seem to be more readily responsive to improvements in drinking behaviour. Generally, high correlations were found between alcohol consumption and behavioural impairment at intake and follow-up (viz. 0.69 and 0.68). However, highly problematic drinking was found, so perhaps these figures only hold good for high consumption groups. The two main conclusions of the first Rand Report were that people often give up alcohol without treatment, and also that a return to "normal" drinking was possible for some.

For a commentary and selection of criticisms made on this 1976 report, see the 1978 edition published by Wiley which includes relevant material as an appendix (Armor et al 1978). Criticism of the report centred primarily upon the methodology and concerned the relapse analysis, and whether it had to do with sample-size, length of follow-up, the "narrow

window of observation", and the validity of self-reports. All of these issues and more were confronted and tackled in the second Rand report which I shall consider next.

The Second Rand Report

The second Rand study, although somewhat out of time-sequence since it was published in 1980, might profitably be reviewed at this juncture. Entitled, "The Course of Alcoholism Four Years after Treatment", this second report extends the follow-up length of the cohort studied at 18 months to 4 years. It is the most substantial follow-up of its kind ever undertaken to date. It gives a high priority to the solving of some of the methodological problems of the first Rand study and it employs a much wider range of variables to assess alcohol related impairment and mortality. It uses much larger "windows" through which to observe drinking behaviour at six months and four years and it achieved an excellent response-rate of 85%. However, it needs to be borne in mind that when considering generalisability, the figure of 85% represents a completion-rate drawn from the 18 months follow-up, which itself had a rather less satisfactory response-rate of 57.8%. If the original sampling-frame is used, that is to say, those clients who entered the study in 1975, then the response-rate for the purposes of generalising the data is therefore much less dramatic. There were two independent studies of client self-reports. The response-rate of 85% produced a sample of 500 individuals who could be studied over a period of between 18 months and 4 years after treatment. The validity of self-reports was checked by collateral evidence and a check upon

blood-alcohol concentrations. These procedures did indicate some under-reporting but not enough to alter the study results.

The definitions of remission were tightened up, non-problem drinking (formerly "normal drinking") did not permit any symptoms of dependence at all. Because the 30-day window of the 1975 report was extended to six months in the Second Rand Report, the short-term abstinence category was lost; and, what was formerly short-term abstemiousness, became non-remission (as indeed was generally found to be the case in any event).

The final outcome categories for the four-year follow-up were thus threefold:

1. Long-term abstainers (6 months+)
2. Non-problem drinkers.
3. Problem drinkers in the last six months.

The remission-rate on the four year follow-up was 46% whereas at 18 months it had stood at 67%. The 46% is comprised of 26% longer-term abstainers and 18% non-problem drinkers. A much improved design enabled the researchers to undertake a far more comprehensive relapse-analysis.

The second Rand study was not an experimental study, nor was it a treatment evaluation in any strict sense. "The follow-up study was not designed as an experimental study of alternative treatments, but its detailed history of treatments over four years did permit some analysis of treatment relationship. ... The results are consistent with much of the scientific literature on alcoholism treatment and our previous

study."

The relationship between amount of treatment and outcome still held that those who had over five visits to an Alcohol Treatment Unit showed a "slight" advantage. Again it was demonstrated that type of treatment made no difference. The data on treatment history over four years are complicated, as indeed are the data generally at four years.

It appears that repetitive treatment in either formal or informal settings (i.e. Alcoholics Anonymous) is the rule, even for the non-treatment group who had not entered treatment at the initial 18 months follow-up. "Historical data indicates that many alcoholics, even those who do not enter treatment at a particular point, are involved in a recurring pattern of treatment, remission, and relapse."

An analysis of the study survivors produced interesting data. Problem drinking rates for previous long-term abstainers were 30%. For short-term abstainers the figure was 53%, and for previous non-problem drinkers it was 41%. (The difference between 30 and 41% is statistically not significant, although that between long and short-term abstention is). Fine-grained analysis was done on the relapse data. One of the most interesting and, from a clinical point of view, important results emerged from an analysis of different client characteristics as they react with abstinence and normal drinking. A regression analysis indicated the extent to which the background variables such as degree of dependence, socio-economic status, marital status, age and race were associated with relapse-rates at 4 years for

those who had been in long and short-term remission or in non-problem drinking groups at 18 months. The relapse rate was not uniform across these three groups of clients, the three remission categories varying substantially according to the individuals' previous drinking history and background. Overall it was found that "Among people who were under 40 years of age, unmarried, or less dependent on alcohol at admission, the rate of relapse for non-problem drinkers was equal to, or lower than the rate for long-term abstainers. Hence it appears that for some alcoholics, especially those under 40 yrs. and less dependent upon alcohol, non-problem drinking can be regarded as a form of remission". The Rand researchers stopped short of recommending non-problem drinking as a treatment-goal for this group. No doubt this omission on their part was influenced, to some extent, by the furore that greeted their first report.

This important finding has not gone unchallenged. Two statisticians, Smith and Jackson (1982) argue that the amount of variance explained by the statistical procedures used is only 13.5% for the under 40s group and 7.2% for the over 40s group. They argue that "as a rule of thumb" the "physical sciences R-squared statistic is expected to be greater than 90% for significance to be acceptable; the biological and economic sciences require it to be between 50 and 80%, whilst the social sciences (as is the case here) will usually accept as significant, an R-squared in excess only of 25%. The statistical model developed from logical regression analysis is not significant enough to convince us that this

result will necessarily hold up under scientific scrutiny, to wit, an experiment designed to replicate the results..... and like Polich et al we hope it will be given a more rigorous test in future work." (Smith and Jackson 1982) This is an extremely important finding for alcoholism treatment and future evaluations since as yet these findings have not been replicated. The researchers stress that their data are observations of alcoholics' behaviour "in a non-experimental environment". Experimental matching would be needed to resolve the issue.

The overall results of the remission-analysis are uninspiring: "When the results of the different follow-ups were combined, they found 13% of the sample classified as long-term abstainers at both follow-ups. Another 9% were classified as non-problem drinkers at both follow-ups, and an additional 6% had shifted from abstention to non-problematic drinking or vice-versa. Thus, altogether, 28% of the sample were classified in a remission status at both time-points. This represents an improvement since the time of admission to treatment, but it also indicates that most sample members did not achieve long-term stability". They recommend that long-term remission should not be considered an appropriate criterion of treatment outcome and that instead a total length of time spent in remission ought be adopted, all in all a fairly pessimistic state of affairs.

A final point deserving attention before passing to the next landmark study of the decade concerns the theoretical issue of "loss of control", which of course relates to the

clinical matter of drinking goals. The Rand data contradict "classical" notions concerning "loss of control" (e.g. that even one drink will lead to over-indulgence and loss of control over drinking). The data also repudiate later, modified versions of the loss-of-control theory which Armor (Armor 1981) has described as "The Revised Abstinence Theory". This view of loss of control posits a delay between first drink and the eventual lapse into uncontrolled drinking with perhaps a phase of controlled drinking preceding the eventual relapse; hence the continuing need for abstinence. This modified loss of control theory is credited to Keller (1972) and Ludwig and Wikler (1974) in some measure.

The theory predicts that moderate drinkers at the six months follow up should have a higher relapse-rate at 18 months follow up than would long-term abstainers. Contrary to such predictions, no such differences were found in the relapse-rates between the two groups. At the four year follow up, the new pattern of remission and relapse which was influenced by age, social stability and level of dependence was observed. This does not substantiate the revised abstinence theory either: a very severe blow to "traditional" theory. In the 4 year study we are given a rather different perspective from which to contemplate these results such that the traditional notions concerning loss of control, a cornerstone of the disease theory, become redundant. None of these results, with the possible exception of the detailed relapse-analysis suggesting the crucial interactions of client variables in the determination of the various outcome-

categories, were novel.

Similar results had been reported over a period of two years prior to the publication of the first Rand study (Hodgson 1979). It would appear that non-scientific factors would have to be invoked to explain the controversy caused by these two reports but particularly the first study. Political factors surrounding the organisation and funding of treatment and research especially in the United States do help to shed light on what Professor Cahalan saw as "The alcoholism field acting like a ship of fools" (Cahalan 1979). In addition to this, and over a geographically larger area, the ideological division between the craft model and the scientific model employed by treaters, as described by Kalb and Propper (1976), helps us put these events in an historical perspective and to make sense of the perturbations of the 1970s decade, particularly with respect to the United States. I will have cause to return to these contextual issues in the third section of this review, when I will argue that these contextual matters have to be taken account of in any attempt to make sense of the reviewed research from the preceding decade and, indeed that an explanation of the results of research into alcoholism treatment can only be understood if one gives more than a passing glance at the restyled institutional interest and wider historical context. This whole arena makes rather a good example of the myopia described by philosophers of science who have written about the theory-dependent nature of observation.

For the next landmark study of the 1970s, We cross

the Atlantic to the Institute of Psychiatry at the Maudsley Hospital where the controversy began in the first place.

The Maudsley Study

The Maudsley Hospital "treatment versus advice" study constituted a methodological watershed in alcoholism treatment studies (Orford and Edwards 1977). This study was as good an approximation to a valid no-treatment control group as the alcoholism research world had encountered before or since. Its findings, together with those of the first Rand study discussed above, left the alcoholism research and treatment community in a state of consternation. The reason for this state of affairs was the study's overall finding that treatment of an extensive sort worked no better than a single extensive three-hour session of advice to the patient and his spouse. In a paper recording follow up results at one year (Edwards et.al. 1977) the researchers concluded that their paper "should not be interpreted as a manifesto for pessimism", but nevertheless it is probably the single most important and compelling cause for pessimism in the alcoholism treatment community, reaching its peak in the last quarter of the 1970s decade.

The object of the research was to determine the value of a therapeutic regimen which might "fairly represent the average package of help that a well-supported treatment centre anywhere in the Western world would to-day offer the alcoholic who enters its doors". The Maudsley programme with its psychiatrist and social worker available for each couple is said to be comparable with the "more privileged end of the

spectrum of North American clinics". These are substantial claims indeed and clearly a treatment evaluation from such a centre must demand attention, particularly when the two principal researchers, one a psychologist, the other a psychiatrist, are both of international standing.

The design of the Maudsley study was both simple and elegant. Having established a brief list of exclusion criteria of the usual sort, to do with brain damage and psychiatric disturbance, all consecutive referrals of married men attending the alcoholism family clinic (itself something of an innovation in the 1970s) were randomly assigned to either the treatment or the control condition. The sample consisted of 100 men who had been referred to the clinic by various community agencies. All subjects were aged between 25 and 60 years and lived within a reasonable travelling distance of the Hospital. The research strategy was tripartite :-

1. An initial assessment for both groups.
2. Allocation to treatment or no-treatment group.
3. Follow-up.

The initial assessment given to both groups was the same. It was in fact much more intensive than would be given by most agencies, and lasted approximately three hours. A history was taken by a psychiatrist using a semi-structured interview. A medical examination was conducted. The patient was then seen by a psychologist who administered various tests concerning the usual range of psychological constructs such as for personality and self-esteem. In addition since this study had to do with marriage as a major focus of attention

(the authors in fact described it as a study of marriages), marital relationship measures were taken as well as perception of the drinking problem. Whilst the man was being interviewed, his wife was at the same time being seen by a social worker who took a personal history and completed various measures relating to the wife's coping style and marital interactions, etc.

Once the interview data were collected, assignment to either the treatment or advice group was made. The assignment procedure was quite rigorous; a randomisation table was used, the sample being stratified into six subsets accounting for three levels of occupation and two levels of symptomatology. Each subset was assigned by separate randomisation procedure. The whole process resulted in a quite satisfactory matching of the two groups.

After assignment, both spouses were interviewed in a joint session with the three workers involved. This initial counselling session also provided further assessment data on how the couple interacted with one another. (Various papers report the subsidiary focus on alcoholism and marital interaction). In each case the psychiatrist pronounced the diagnosis of alcoholism, advised total abstinence, and urged either a continuance of, or return to work as the case may be. He also urged them to make greater efforts towards marital cohesion and harmony. This initial counselling session concluded with a more far-ranging discussion with more individualised interpretations given on the basis of individual assessment.

On completion of the feedback from the initial counselling session, the advice group (control) were advised that the solution to their difficulties lay in their own hands, and that they should go away and act upon such advice as they had been given. If the patient were to suffer withdrawal symptoms, he was to seek the help of his general practitioner. Although the patient would not be offered any further clinical appointments, he was nevertheless told that someone would call each month to see the wife and to monitor progress, the social worker who made the monthly visits purposely keeping therapeutic transactions at arm's length. A great deal depended upon this non-therapeutic intervention to the extent that no therapy was enacted (however indirectly or inadvertently), such that the results could be attributable solely to the initial assessment procedure and the single session of "directive counselling".

Overall, the advice group received an excellent assessment and an ideal counselling session - but perfunctory monthly visits thereafter. The treatment group were introduced to Alcoholics Anonymous; they received "Abstem", and medication for withdrawal problems where necessary. A further appointment with the psychiatrist was made, who worked out individualised treatment plans for his patients, whilst the wives were seen by the social worker. The treatment continued on an out-patient basis with the goal of total abstinence. Again, strong emphasis was placed upon work directed towards marital cohesion; if out-patient treatment proved ineffectual, then a stay in a specialised in-patient

alcoholism unit was offered, which involved in-patient therapeutic groups and occupational therapy lasting approximately six weeks. All of this amounted to a very flexible treatment response. The therapeutic involvement was initially intense but became less so as treatment proceeded.

The study was designed with a time-base of 12 months, and a second follow-up was done at 24 months; but in some senses this second follow-up constituted a separate study. The strict separation of the two groups could not be maintained over a two-year period; many effects that might have been categorised under the heading of "history" would have inevitably weakened the impact of the design over the space of the second year. The authors of the report particularly mention the seeking of help and the loss to follow-up as major impediments to overall validity. No attempt was made by the research team to deny treatment to members of the advice group during the second year. They suggest that the second year ought to be seen as a "free wheeling period, with patients making their own choices", and, interestingly, there were "relatively few families engaging in very active therapy". The experimental design therefore refers to the 12 month follow-up; i.e. the randomised controlled trial.

The completion-rate at 12 months was 46/50 (92%) for the advice group and 48/50 (96%) for the treatment group. The main outcome measure consisted in data concerning the husbands' drinking over the study-period. The initial assessment enquired about the number of days spent drinking

and the amounts consumed. The follow-up described each of the 52 weeks according to the highest amount of alcohol consumed during that week. The wife's reports of the husband's drinking were noted as either acceptable or not acceptable for each of the 52 weeks. In none of these reports of drinking behaviour was there any difference between the two groups over the study-period. (Adjustments were made to the data where necessary to take account of those patients who spent time in hospital). In addition to the taking of accounts of drinking from the wives, each spouse was asked to give her subjective rating of the extent of the drinking problem experienced by her partner, and to comment upon any improvement thought by her to have taken place over the 52 week period of the study. No differences were found to have emerged between the two groups on this criterion. Husbands tended to be more optimistic than wives; at 12 months "about one third of patients across both groups had slight, or no drinking problem".

Social adjustment was measured in terms of time spent off work, sick or unemployed; (unemployment rate at the time, and in the area of the study would have been at around 2 or 3%), and marital situation. Again there were no real differences between the two groups, time in hospital having been controlled for in the treatment group. Once again, husbands gave rather more optimistic reports of improvements in their marital problems than did their wives. Unlike the findings of the Emerick and the Rand studies, no evidence was found by the Maudsley team to support the notion that the

amount of treatment was correlated with outcomes either in the treatment or in the advice group. This conclusion was demonstrated by a detailed analysis of help-seeking behaviours in both groups.

Of particular interest in this study is that part of the analysis which dealt with "Patients' views on what had helped them". The relevant data were collected by giving the patient a check list of items assumed to be relevant for recovery. Both groups were in accord with the rank ordering of their choices in this respect. The most important factors nominated were unrelated to those of formal or informal helping agencies, i.e. The clinic or Alcoholics Anonymous. "What might be deemed the elements of the overt package of help are being seen as less helpful than the three items relating respectively to, changes in external reality, intrapsychic change, and change in the marital relationship. Among the four top-ranking items, only the single session of directive counselling was nominated from the pool of relevant treatment items. "The conclusion must be that patients in both groups were generally rather unimpressed by any helping intervention other than the initial counselling, and subsequent efforts by the family clinic certainly were not rated specially highly."

What then were these valued changes that patients found more beneficial than formal treatment? These are listed in both sources cited as changes in external reality e.g. work, housing. The study reported on a number of complex analyses concerning "marital and other socio-environmental

factors, and treatment outcome". The data suggest, in line with numerous other studies, that marital cohesion and support is predictive of good outcome except for those people with higher occupational status or higher self-esteem. Another way of saying this is that the study highlighted four sets of marital and socio-economic correlates of treatment outcome. These four sets of variables were :-

1. Marital variables.
2. Intake occupational state.
3. Husband's self-esteem at intake.
4. Wife's hardship score.

Occupational status was strongly predictive of a good outcome - this variable also affected the relationship of marital cohesion and outcome since the latter (or rather its absence) was not predictive of poor outcome given that high occupational status obtained. By combining sets 1-3, outcome was highly predictable. The hardship scale was a list of ten distressing family events. There was a definite progression from low to high scores that mirrored poor or good outcome as the case may be. Husbands who had low occupational status, and whose wife had a high hardship score, had a "particularly poor prognosis". This was aggravated by the husband's poor self-esteem. The opposite was the case where the prognosis was good. None of these predictor variables was independent of lack of cohesion - the most predictive of the four variables for husbands who were not in the high occupational status group, nor was lack of cohesion in marriage predictive for husbands with above average self-esteem.

It would appear that marital cohesion strongly interacts with self-esteem to produce good outcomes, or alternatively other social resources like high occupational status can compensate by providing a buffer against stress such that self-esteem is unaffected. Also, self-esteem as such may have been robust because of some other social resource in the individual's social environment, not taken account of in the study. Where the individual feels good about himself and his family, for whatever reason, outcome would appear in such cases to be good. A check that this is not illusory on the part of the husband, is that it is supported by the corresponding hardship score for the wife. If family hardship is in evidence, and family relationships cannot provide a sense of self-worth, there always remains occupation for those fortunate individuals who derive a strong sense of self-worth from their job-status. The data are quite complex in these analysis but there is a strong unifying, commonsense perspective which is of considerable interest. The implication is that what might be called curative factors, the major determinants of recovery, are related to activities and relationship outwith the treatment situation; not only this but more conventional psychological constructs thought to be of relevance in treatment can be viewed as being in interaction with all these extra-treatment phenomena.

The second year follow up consisted of "relatively short enquiries, focusing particularly upon the patients' drinking behaviour over the preceding 12 months and on any evidence of them having drunk in a controlled fashion". Data

were collected from husband and wife separately by a shortened version of the same questionnaire used at the first 12 months. Sixteen couples were lost to this second follow up.

The results are displayed in the form of a cumulative relapse-curve. In terms of displaying drinking behaviour per se, their format gives little information at two years since "all but 10 of the 95 had drunk within the first four months of the initial consultation." By one year only eight men had abstained totally according to their wives' reports, and at two years this figure had dropped to two. (The authors add that both of these men were known to have drunk subsequent to the second follow up interview). Since consumption per se need not necessarily be a problem for the wife or the patient, the relapse curve also described reports of wives when drinking was considered to be unacceptable, . Only twenty were reported to have drunk in an acceptable fashion on at least one occasion four months after initial treatment. This figure fell to 12 by the time of the 12 months follow up, and by two years it had dropped to eight. The authors do indicate that a single instance of unacceptable drinking, not followed by a continuance of problem drinking, is still enough to influence the direction of the cumulative relapse-curve, and that various definitions of relapse would produce differing curves.

This fact is demonstrated by a third curve which described reports from wives of an accumulation of ten unacceptable drinking days. Here the figures were 56 at four months, but by the first anniversary, only 24 had not been so

described by their wives, and by two years there were 18 men who had "not relapsed", where relapse was defined as an accumulation of 10 unacceptable (to the wife) drinking days. Not a single individual was able to follow the advice given, to the letter, that is, abstain totally, which was the message that was given at the initial session. By about four months only 10% had remained fairly stable for most of the year, but by two years this fell to 2% and, indeed, to zero after two years. The authors compared this relapse curve with similar curves produced by Hunt and Matarazzo (1970) who accounted for relapse in drinking, smoking and heroin addiction. They all show a rapid relapse within the first three months of treatment and, according to the combined curves, 20% to 30% go on to achieve abstinence. The two year result mirrors those presented by Hunt and clearly support the notion that drug users relapse very quickly. It would appear then that the data on relapse fit very adequately with a comparison with other habit problems.

The drinking outcomes were classified as good, bad, or equivocal. Good was defined as the wife reporting five or fewer weeks containing any episode of unacceptable drinking, together with a husband's report of five or fewer weeks when he consumed over 200 grams per day drinking. Outcome was deemed bad if the wife reported 6 or more weeks where drinking unacceptable to her had occurred, and if the husband reported 26 or more weeks where drinking more than 100 grams per day had occurred. All other cases were either equivocal because the spouses did not agree either on the frequency of drinking

or the amounts consumed. There was a reasonable amount of stability over the whole study period inasmuch as over half of the cases remained in the same outcome category at 12 and at 24 months. The majority of change was from or to the equivocal category. This observed stability is not generally found in work reported above or elsewhere. As described above, outcome at one year could have been predicted on the basis of the wife's hardship score and marital cohesion; and the husband's job status. These predictors are also valid for good drinking outcome at two years. This was particularly so for the wife's hardship score, but less so for marital cohesion and job status.

Of the 26 men who had a drinking outcome categorised as good at two years, 11 were found to have been virtually off alcohol over the second 12 months, whilst 10 men had continued to drink in a controlled and acceptable fashion. Eight of these 10 husbands had drunk in a controlled way from nearly the beginning of the first study year. The controlled drinkers tended to have lower dependency ratings. This of course, is what a lot of research has found (See Heather and Robertson 1980).

A finding not easily explained was that the controlled drinkers tended to be those individuals who were most confident about abstaining at intake. In their recommendations, the authors of this report recommend that in general treatment should be less "interventionist" than is usual. They outline a basic model which keeps treatment process out of their research design, namely that treatment

should consist essentially of a good assessment which is discussed in a counselling session with a subsequent follow up session to check on progress. They suggest that specialist in-patient units are unnecessary. With regard to future research efforts they suggest that treatment research should not proceed unless "radically new approaches are being tested, rather than minor modifications to traditional regimes". The area for future research which they envisage as being most productive is work on "family and other natural influences". The report concludes with a plea that alcoholism studies should not become too isolated with "too great a ritualisation of therapeutic methods".

There can be little doubt that the Maudsley treatment and advice study, coming as it did from one of the most prestigious research institutions in the field of Alcoholism and authored by internationally famous workers, would have an immense impact on the world of alcoholism treatment and research, as indeed, did the Davies' paper before it. Clearly, the sense of pessimism that I have alluded to as in some respects characterising alcoholism treatment and research during the 1970s was heightened by this study. It also produced more specific shock-waves amongst Alcoholism researchers and clinicians as many of the reviews which appeared in its wake indicate. It was feared for instance that government funding agencies might read the report and conclude that treatment does not work and thence cease to fund treatment agencies. Certainly, it is worth remembering that another defining characteristic of the alcoholism field during

the 1970s, was the emergence of a whole army of professional and paraprofessional Alcoholism counsellors, who were dependent upon alcoholism treatment for a living. From the more focused interest of this review chapter, the most salient results of the study are :-

1. The overall failure to find a superior treatment effect.
2. The extra-treatment effects nominated by the couples in the study as being most helpful.

About the same time as the Maudsley study, another treatment review by Blane (1977) produced similar overall results. Blane reported low sustained abstinence-rates for both treated and untreated individuals, but also that improvement in drinking behaviour occurred in about 2/3 of cases treated, as well as high rates of improvement in those who had had little or no formal treatment. Also, in keeping with previous reviews, desirable client characteristics were highly correlated with good outcomes.

Like the Rand reports, the Maudsley treatment and advice study attracted wide attention and responses in the academic journals. Kissin (1977) detailed the limitations of the study, a task which had been done quite adequately by the authors themselves in their Maudsley monograph. He also complained that the study had a negative rather than a positive perspective. Kissin and others were concerned about the impact of the study on policy-makers and others responsible for programme funding. Glaser (1977) highlighted the need for more appropriate designs in alcoholism treatment evaluations, to account for the multi-dimensional nature of

the problems observed, in his review of the Maudsley study.

In 1977 Clare (1977) produced a much discussed review that questioned the same basic assumptions of alcoholism treatment. Clare reviewed most of the work already discussed in this section and drew similar conclusions, namely, that small numbers of individuals achieve abstinence regardless of the type of treatment they receive. A still smaller proportion manage to sustain non-problematic drinking. No differences are apparent in treatment methods, and therefore there is no justification for expensive treatment enterprises. Perhaps most interestingly, Clare concludes "those few studies which have attempted to analyse outcomes in minimally-treated or untreated alcoholics suggest a high rate of so-called "spontaneous remission", perhaps as high as 50% in some cases". This figure of 50% is something of an outside estimate in the literature. With regard to treatment, the theme of pessimism is reinforced. Clare had somewhat provocatively entitled his paper, "How good is treatment?", which of course suggests the response "not much".

Referring to his list of six conclusions, Clare comments that "such conclusions represent a meagre return for the exertions represented in clinical effort and research activity in the treatment of alcoholism over the past 20 years". Then he distances himself from this point of view somewhat strangely, given his very articulate discussion of the work he reviewed and the demolition job done in the paper on treatment. "Such pessimism would seem to be misplaced" he adds. What the conclusions really indicate is the need for

rigorous assessment of various treatment components and the abandonment of complex and expensive treatments if they "cannot earn their keep"; better research methods and research to determine who can and who cannot, benefit from controlled drinking. Laudable though these objectives are, Clare hardly refutes his own conclusion with them. The concluding sentence reads, "The case in favour of significant effects of treatment in alcoholism remains to be conclusively established".

Clare's paper, though not as influential as his then colleagues' treatment and advice study from the Maudsley, has been widely quoted and might be construed as in some sense indicative of the attitudes of the alcoholism field at the time among researchers, at least within the United Kingdom. The two primary attitudes that emerge from this paper are pessimism and ambivalence. (Ambivalence in the sense that there is an unwillingness to adopt a wholly nihilistic perspective, but the data are obstinately negative in respect of treatment efficacy, and the dissonance is managed by a hope for better things to come.)

What I think also is evident in Clare's paper, and indeed in much of the work that I have discussed, is a reliance, from a philosophico-methodological stance, on more of the same: empirical sciences will eventually come to the rescue. In the same volume that Clare's paper appeared, Kreitman (1977) outlines some of the confusions which make for such a state of affairs. From a methodological point of view, he contrasts the incommensurable demands of the clinician and epidemiologist. He pinpoints a particular difficulty in that

"Many of the items counted as problems appear to be reflections of a social context as much as the behaviour of the individual. The context has entered into the definition of the problem. Such confusion of dependent and independent variables is the most potent ground for dissatisfaction with the present state of knowledge".

This is a wonderfully succinct statement of the state of play in the alcoholism field. I think that one can derive explanations for the sense of pessimism and optimism for the future that is evident in the literature from the 1970s from the methodological fix outlined by Kreitman. This issue is pursued later. It is sufficient to note at this point in the review that the data as operationalised by the empirical methods available, lead to a good deal of dissatisfaction and attendant pessimism. The crucial issue of course is that if research methods demand a clear demarcation in the world between dependent and independent variables, is it then the case that the philosophical and logical models which underpin the methodology are inappropriate for the study of social life? Must the research methods be changed?

Whilst still considering work undertaken in the mid 1970s two further reports might be mentioned. In 1976, Filstead et al edited an interesting and much quoted book entitled - "Alcohol and Alcohol Problems: New Thinking and New Directions " - it is instructive to note that this is the second major text of the period which embodies the phrase "New Directions" in the title. Chapter 7 is written by Marc A. Schuckit and Don Cahalan and headed - "Evaluation of

Alcoholism Treatment Programs". It is a very helpful and concise guide to evaluation of the "how to do it" variety. They discuss their impressions of the state of the field and by way of introduction they tell us "Evaluation like the so called "Policy sciences" has taken on some unfortunate ritualistic and cultist characteristics that tend to lead either to undue mystification and complexity in approaches to evaluation or to undue narrowing in the field of enquiry", the latter to suit the needs of politicians and programme funding administrators. Evaluation they see as being primarily undertaken to assist clinicians. Recalling treatment reviews from 1941 onwards they conclude "our assessment of the literature leads us to conclude there is a ground for great concern because so little sound and clinically relevant information exists on the effects of treatment". They discuss spontaneous remission and some of the usual explanations of it. Again we encounter the rule of one third - they tell us "Most programs, irrespective of their therapeutic interests, facilities or enthusiasm, report a success rate of 25 - 30%". They make the important point that "Because spontaneous remission and improvements are common, it is important to understand the natural course of untreated alcoholism before one can evaluate the effectiveness of therapy". Further, "as all outcomes are relative - the patient improves compared to what? - the use of controls is imperative". They refer to various studies to demonstrate the ubiquity of biased sampling with its malign impact on validity. Illustrating their point they quote a phrase from Wallerstein (1957) who reviewed the

literature and found "a confusing web of inconclusive claims and evidence". We are told that "In addition to lack of controls, most studies used imprecise definitions of improvement or relied on abstinence as the sole criterion for determining outcome". They are also wary about the dangers of comparing disparate groups in treatments because of the differences in demographic and prognostic indicators, particularly hospitalised patients who might be in a chronic phase of illness. Warnings are also given against new treatments launched with enthusiasm and sometimes "touted as cures". Drug treatments are instanced (No drug has ever been found to be superior to a placebo in a correctly controlled trial).

All these flaws to be avoided are commonly encountered in the literature. Schuckit and Cahalan press home the message that self-deception is usually predicated in the guise of "Clinical experience, collegial encouragement and patient testimonials". Such self-deception can be avoided by using scientific procedures. Treatment goals must be enumerated at the outset of treatment, otherwise any desirable changes might be claimed as treatment successes. They report a review of treatment programmes which revealed that only 10 - 15% stated their objectives - even at the time of evaluation. These two accomplished researchers catalogue a list of flaws which reflects very poorly on the Alcoholism Evaluation Research endeavour up to the mid 70s. Their purpose in producing such a criticism is of course to enlighten neophyte researchers so that they can identify and avoid such flaws in

their own work; they are encouraged to follow the guidelines and procedures laid out in the main body of the chapter. A little further on they tell us that the "discussion of evaluative techniques below is compatible with the precepts from those authorities in other fields [referenced previously], in that the primary emphasis is placed upon the need for demonstrable objectivity, and upon the canons of scientific research, including such principles as facilitating replicability of studies, by other observers, careful definition of goals and methods, and explicit procedures". In this long sentence there are no details to quarrel with, good advice indeed. However, as the adjectival clauses pile one upon another and in the general narrow focus of the piece, one can sense a shade of scientism, submerged and requiring an intuitive act of interpretation on the part of the reader, but nevertheless present, in the message of that particular passage and also in what follows. The message is that despite the unhealthy state of the alcoholism treatment evaluation research, the ailments are not chronic, they are primarily due to poor research practice, the most heinous and prevalent ailment being either no or inappropriate control groups. This state of affairs can be remedied: the prescription is more of the same only better executed operations. This is very much the second option mentioned above. I take this mode of thinking about Treatment evaluation in the alcoholism field, a strong faith in the curative properties of sound empirical science, to have been widespread in the 1970s. Again at root there seems to be a problem to do with a failure to make

distinctions between theory and method, such that one never gets beyond the methodology to raise interesting conceptual issues which in turn might have an impact upon methods.

The importance of spontaneous remission as a benchmark against which to make sense of the analysis of treatment evaluation outcome data was raised above by Schuckit and Cahalan and was treated at some length by Tuckfeld from a somewhat different perspective. Spontaneous remission is a somewhat mysterious medical term and for methodological purposes the mystery need never be unravelled. It is merely a functional number; a rate against which outcome figures can be compared - that is they define data arrived at in treatment outcome studies such that they assume meaning for the reader if a result is worse, the same or better than spontaneous remission for comparable samples. This is a mathematical function. However spontaneous remission embodies a fascinating human activity for study. The question for researchers is not only how often does it occur?; but how does it come about? What are the curative influences and experiences at play in an individual's everyday world, that singly or in interaction, induce people to recover from their drinking problems? That was the sort of research Tuckfeld undertook which was discussed earlier. This process research is crucially important in my view since it is quite evident that the vast amount of treatment research does not provide an abundance of insight into what the curative factors are.

Looking briefly at the head counting aspect of spontaneous remission first, it is evident that there are

degrees of remission just as there are degrees of recovery in treatment. A lot depends upon what is meant by remission: a theoretical issue; and also on how one measures and analyses one's data: a methodological issue.

One aspect of spontaneous remission which has been acknowledged in the literature for a long time is that the incidence of alcoholism within a population decreases with increasing age when mortality is controlled for. This is the "maturing out" process, a phenomenon referred to by a number of researchers (Drew 1968; Cahalan and Room 1972; Zimberg 1979).

In a major report Roizen et al (1978) confront some of the methodological issues in assessing the "existence, frequency and character of spontaneous remission from alcohol problems". They demonstrate how differential criteria of alcoholism and of what counts as remission affect estimates of spontaneous remission. At the time of writing (1978) they characterise the then current state of knowledge of spontaneous remission as being a "weak and uneven collection of studies." Among them one can find some evidence to support expectations of very low or very high remission. For instance, Kissin et al (1968) reported a rate of only 4% in an untreated control group, while Goodwin et al (1971) found a 40% rate in a sample of offenders followed up at 8 years. Clancy et al (1965) reported a rate of 54%. These studies all varied greatly in terms of the samples they drew from, the diagnostic criteria used and outcome measures. Nevertheless, what literature exists on the subject reports that remission

in the sense of 6 months of abstinence can be expected in 15% of the cases and remission in the sense of some improvement in about 40%.

The survey by Roizen et al (1978) was designed to obtain a prevalence rate and to identify what factors are involved in spontaneous remission and their relative importance. The survey was a two wave panel study with white males aged between 21 and 59, the second wave being completed over a period of 4 years. An indication of the confused state of affairs arose for these researchers at the outset for they were unable to classify their sample into alcoholics and non-alcoholics. They explain that it was not possible to divide the sample by applying a consensual set of diagnostic criteria to the scales used because "such diagnostic expertise is nowhere to be found". Their data did not describe a bimodal distribution on the problem scales. "Our samples have consistently revealed continua on these problem dimensions", nor were there any comparable data for them to make comparisons with. Screening tests, they say "tend to employ disjunctive sets of criteria that would yield positive diagnosis for an uncomfortably large selection of the male general population".

Alcoholism criteria are treated in this study as a variable, hence they have differing results for differing cut-off points on their scales. They present data for various cut-off points at Time 1 for problem drinking and Time 2 definitions of remission rates. This generates multiple spontaneous remission rates (only one individual from the

entire sample was abstinent for a year). Only a little over half of their sample reported not having "at least one" minimally severe drinking problem, because such a substantial proportion of the population experience some degree of problems because of their drinking, it follows that in the United States at least, a remission criterion of "no current problem" is a very stringent criterion indeed. The general picture is that having a problem at Time 1 incurs a strong probability of having some problem but not the same problem at Time 2. The main question they consider is the rate of spontaneous remission in a general population of sample adult males and they argue that if abstinence is the criterion the rate is zero. Different criteria of measurement would permit different researchers to claim improvement rates as low as 11% or as high as 71% with these same data, depending on the cut-off points. It seems that the establishment of a single remission rate for this sample is not quite so pertinent as an appreciation of the swing on the remission rates differing from Time 1 and outcome measurement criteria. The authors point out that remission here "touches on different conceptual and practical issues in the field of alcohol problems each requiring somewhat different approaches", and that there is "no single remission research problem but a number of problems, each requiring somewhat different approaches".

This study is important for the way it illustrates the complexity and multiple problems associated with the issue of spontaneous remission. To date it is the only such study to look at these issues in an empirical fashion and

underscores the inadequacy of a unitary model of alcoholism. They conclude that their results support those of Emerick (1975) discussed previously, and that "By most criteria there was a substantial amount of spontaneous remission" This suggests that the conventional clinical picture of drinking problems as a relatively stable and lasting phenomena may need changing. Instead we might picture a great deal of episodic and situational flux in the relatively large fraction of the population that ever drinks enough to risk a drinking problem.

Another widely quoted review of spontaneous remission is that of Smart (1975) who makes the point that spontaneous remission means "without professional treatment" and not "that remission occurred for no reason at all, that is, it is unexpected and strange". The first study of spontaneous remission located by Smart was that of Miller, completed in 1942 (Miller 1942). In total he found 39 studies to review between 1942 and 1975 as well as three survey reports that bear on the subject. Smart makes the point that definitive statements about spontaneous remission are difficult to make because information about it comes primarily from "alcoholics not applying for treatment because they realise (correctly) that prognosis is good". What he found in terms of reported rates for spontaneous remission was "overall rates vary from 10% (Newman 1965) to 42% (Goodwin et al 1971) for alcoholics not seeking treatment. Yearly rates vary from 1% to 33% considering all types of studies."

Though the head counting is important for its function as a baseline, more productive from a theoretical and

treatment perspective are those studies that address themselves to how recovery comes about. This aspect of spontaneous remission studies is in the tradition of Tuckfeld's work discussed above and bears on the main focus of this thesis.

A Scottish study by Saunders and Kershaw (1979) identified problem drinkers, remission and any characteristic features associated with it, in their sample survey of drinking in Clydebank. The study was flawed from the point of view of a valid prevalence figure for spontaneous remission since the figure they arrived at, 1.7%, was acknowledged by the authors as being unreliable. Their spontaneous remission sub-sample in addition to being asked if their drinking habits had changed, were also asked to say what had brought about the change. The reasons given by the 41 "past problem drinkers" detected were in rank order:

1. marriage
2. job change
3. physical illness
4. family advice
5. financial
6. General Practitioner advice
7. ageing

The reasons given by all the sample for changing their habits were very similar. Data were adduced to indicate that cases of spontaneous remission had rather less chronic alcoholism as did those who had experience of treatment. They found that the no treatment groups took twice as long to stop

alcohol than did the treated group. As far as the results are concerned the major inference is the apparent effectiveness that life circumstance changes have in altering what are often thought to be fairly immutable habits. They also make the interesting suggestion, no doubt with the hectic drinking practices of their study area (Clydebank) in mind, that "for many individuals, problem drinking or alcoholism is a subculturally normal behaviour from which the majority "mature" without any lasting negative consequences" (Saunders and Kershaw 1979).

It is evident then from a brief review of these few studies that spontaneous remission is an enormously complex subject which embodies all the crucial research questions both theoretical and methodological we need to ask of alcoholism treatment, both its process and its outcome. Nevertheless, from this much underworked research domain productive new insight may come both in the sphere of theory and methods. But by far the most important aspect of spontaneous remission is just exactly what it is and how it comes about.

We have seen how Tuckfeld's model attempts to address this matter and also how others have touched upon it tangentially. Genevieve Knupfer (1972) provided a seminal piece of work in this context and one which perhaps inspired Tuckfeld's research.

Her chapter entitled simply "Ex-problem Drinkers" draws on data from two general population surveys in San Francisco undertaken in early 1960 and reported upon after a lapse of two years. She states: "We found more cases than we

expected to find of people who cut down more or less spontaneously (i.e. without treatment) and people who now drink "socially or normally", whereas their pattern at one time looked like an addictive or diseased pattern." She chides traditional conceptions of alcoholism thus: "There is something about the word alcoholism that seems to lead people to chase up sides and dig their heels. Also I like to think in terms of degrees of severity in this area although many people tend to think alcoholism is like pregnancy; either one is or one isn't."

With regard to incidence of recovery or "reform" as she calls it, she says: "We can say, from the two samples , only that the rate appears to lie somewhere between 19 and 30%", though later in the chapter she gives a revised rate of 26 to 33% using differential criteria. These cases of recovery were scrutinised to see how many were cases of spontaneous remission. She found "the rates are about 25%, which represents at least three fourths of all recoveries." This was a very conservative figure since informal treatment like religious conversion, Alcoholics Anonymous, or medical problems were excluded as well as formal treatment.

In her discussion of motivation for recovery Knupfer says: "In explaining "why they quit" respondents often give strangely trivial reasons". One of the thumbnail sketches she presents is of a man who got irritated by a barman bragging about his new sports car - "the respondent thought to himself, indignantly, "He's buying that car with my goddam money, goddam it, and what have I got?" Then and there he decided to

quit."

Knupfer presents a series of short case studies which she discusses. The overall rate of recovery and spontaneous remission was "around 30% in cross sections - which means it would probably be higher if a cohort were examined at the end of their lives, when the young men sowing their wilds oats have had time to recover". This is a very similar sentiment to Saunders and Kershaw's view about young people passing through a sort of subcultural alcoholism.

The great majority of her respondents were motivated to "function socially and be able to feel concern about their health". In terms of Tuckfeld's model most of the spontaneous recoveries were unwilling to be labelled; "most of them are not willing to see themselves as hopeless bums". Two sorts of problem drinking are in evidence here. One sort consists of individuals who although they get intoxicated still endeavour to remain in control; these people "are really playing with fire". The other group are people whose objective is to get comatose or essentially helpless. Such people would obviously present a difficult treatment problem. Of the former group she says, "They think they can get away with it and often they can, but sometimes when someone describes their behaviour in humiliating terms or when they realise what is going on, they change, they realise that they are not getting away with it."

These cases of spontaneous remission fit easily into Tuckfeld's model of recovery, indeed it is interesting that both reports make extensive use of case histories and respondents' own language which remains very similar across

the two reports. Also the processes reported by the respondents are very similar: viz a refusal to be labelled, humiliating experiences etc. Only a small proportion of Knupfer's overall sample gave any credit to alcoholism treatment for their recovery, and this of course is exactly what the Maudsley study reported also.

In relation to spontaneous remission, Tuckfield asserts "Attempts to quell the debates have usually focused on delineating addictive and non-addictive types of problem drinkers of which only non-addictive pathological would permit of spontaneous remission." Commenting upon the literature on spontaneous remission he says it "may be characterised as epidemiological surveys, as studies of such alcoholic populations as untreated control or comparison groups in experimental studies. Other than Knupfer's brief reporting of "routes to recovery", most of the literature is speculative in regard to the substance of the process or processes". In discussing his study he makes the rather more extensive suggestion that "disengagement from any deviant behaviour is related to external social conditions". He also distinguishes between the labelling involved in the ascription of formal labels by clinicians: i.e. "you are an alcoholic" - the resistance to which is important for his recovery model - and informal labelling involved in the operation of informal social controls, the occurrence of which is important for recovery in his model. "What appears warranted is research that focuses more on the positive features of labelling and their relationship to informal social controls and to social

integration with the legitimate domains of a society".

Ludwig (1972) also presented a statement of reasons given by alcoholics for drinking and abstaining. The subjects were from various facilities who were followed up at four years. The study follows up a sample who were originally studied for an 18 month outcome evaluation (Ludwig et al 1970) which incorporated three monthly outcome measures. What was found by these authors was that by 3 months, approximately 70% had resumed drinking and by 12 months the figure rose to 90% but at any month after 6 months only 55% were drinking, hence a substantial proportion must be going "on and off the wagon". Their reasons for resuming drinking concerned various types of psychological distress and alcohol was used as a sedative drug or to deal with frustration and anxiety related to family life and employment. Ludwig comments that it is apparent from these findings that the reasons patients offer for returning to drink do not support the traditional clinical explanations of craving as a primary determinant. Only 1% had mentioned craving. With regard to abstinence few of these ex-treatment cases offered either their previous treatment or subsequent Alcoholics Anonymous attendance as being helpful in their attained abstinence. Ludwig comments "for the largest proportion of patients (24%) the reasons for attaining sobriety seem to have an almost magical quality in that the desire for alcohol simply vanishes. For the second highest percentage (19%), fear of the consequences of drinking seem to have at least a temporary deterrent effect". The next highest group are described as having non-treatment related

insight, family pressure not seeming to count for much. Ludwig suggests that these results call for a reconceptualisation of treatment both in and out of the hospital - with a more individualised focus.

A further large scale study which attests to the ubiquitous nature of the predictive power of patients' social environmental factors and the lack of efficacy of treatment is that of Smart (1978). This study followed up 1091 individuals from a variety of treatment centres. The rationale of the study came from Emerick's (1975) suggestion that it is unfruitful to search for the best treatment; instead therapists should concentrate on reaching the individual with a programme which "meshes best with his views on the course, nature and treatment of alcoholism". After analysing large data sets for main and interactive effects of patient and treatment variables in relation to outcome the answer to the question "Do some alcoholics do better in some types of treatment than others?" is "Apparently not". Out of a large number of patient characteristics (n=186) and outcome, no interactions were found. Only 6 patient characteristics and 2 out of 6 treatment characteristics contributed significantly to the prediction. Smart's main conclusion is that "The data indicate that patient characteristics are most important in predicting outcomes; that treatment is relatively unimportant and that interactions between the two are uncommon - it appears that the patient's characteristics are important regardless of what happens in treatment". In keeping with this overview of spontaneous remission process variables,

Smart found that patient characteristics "which are most important are directly or indirectly associated with alcoholic symptoms (drinking assessment) and the patient's personal resources (e.g. social stability, employment, positive assessment of life conditions)." It is apparent that alcoholics who have many symptoms but have retained some social stability and supports do best in treatment. One could easily fit an explanation of these results by using Tuckfeld's model. The phrase "positive assessment of life conditions" is an excellent description of positive affect balance, the main theme of this thesis to be explored in to the next chapter.

Interestingly an extensive literature review of "Prognostic Indicators of Alcoholism Treatment Outcome" published a year previously to Smart's paper (Gibbs and Flanagan 1977), substantiates both his main conclusions. Most of their predictors would be subsumed under the rubric "social stability and social class". Perhaps this extremely well executed review which screened studies methodologically for inclusion, is most instructive for the difficulties it reported in the search for predictive variables. After a summary of results the authors ask the rhetorical question "Why doesn't a search of the literature reveal the hoped for list of predictors?". One of their suggestions that patient - treatment interaction might account for it, has as we have just seen, been answered by Smart "apparently not". Referring to their search for prediction variables Gibbs and Flanagan say of the 45 studies included: "Where they reported such information, it was evident that they investigated the effects

of different treatment given to different samples measured by different outcome standards over different follow up periods". Gibbs and Flanagan underscore the diversity of opinion amongst the authors of the 45 studies they reviewed, which together encompassed 55 different treatment groups and 208 different prognostic indicators. They comment that "this diversity of indicators selected by investigators may reflect widely differing theoretical approaches to the problem of alcoholism, or perhaps a helter-skelter approach based on no theory at all".

It is of course important to distinguish between prognostic indicators and outcome criteria. The most commonly used prognostic indicators unearthed by Gibbs and Flanagan are also amongst the most frequently used outcome criteria when multiple outcome measures are used. The same variable is here fulfilling different functions. More complex multiple outcome measures are necessary for the determination of prognostic indicators. This is so because when drinking behaviour alone is used to assess outcome, other sorts of improvement in family life and socio-economic functioning are concealed. This was clearly demonstrated by Hart and Stueland (1979).

Reports of treatment efficacy discussed thus far do little to engender confidence. Either treatment does not work or at least it has not been shown to; if it has been shown to work, it does not do what it sets out to do. All reviews so far have made reference to poor methodological standards. Before considering the last but one study of the 1970s decade, I would make two points to counterbalance the somewhat dismal

parade of treatment incapacity I have been intoning over the last few pages. The first is that treatment samples and general population samples can not easily be compared and the expression "the two worlds of alcoholism" is commonly used in the literature to underscore the difference. It has been estimated in the United States that drinkers in the most extreme 3% of general population samples would be considered non-severe clinic patients. Therefore there would appear to be a crucial selection process at work, with the treatment centres picking up those who have failed to achieve a self-induced recovery or improvement. Some Alcoholics experience enormous difficulty recovering and some of course do not recover at all. However, having said that, many do improve and recover, though such recoveries are generally characterised by episodic drinking of some sort. The second is that I have not considered industrial programmes which are still in their early development in Britain but better established in the United States. These programmes, where by definition the treatment population are occupationally intact and are presumed to be endowed generally with good prognostic indicators, tend to report improvement rates at around 70%. Marc Schuckit (1974) summarises much of what has been said so far: "Clinical evaluations and long range follow ups indicate that one third of alcoholics can be expected to recover - no matter what treatment is used or with no therapy at all. In addition, alcoholics improve their affective state and coping mechanisms on their own with the passing of the crisis that brought them to treatment. Also alcoholism is not a disorder

of constant drunkenness and alcoholics periodically experience days or weeks "on the wagon. At any point in time, therefore, a substantial proportion of our patients can be expected to be temporarily or permanently dry".

The final treatment outcome review study of the 70s decade I wish to consider is that of Miller and Hester published in 1980 (Miller and Hester 1980). It is an extensive book length review of over 600 references masquerading as a book chapter. This review is of historical import for the alcoholism evaluation literature by virtue of the fact that it is the first competent review to affirm the value of treatment. It starts by asking a series of rhetorical questions paraphrasing the literature on treatment outcome i.e., "How effective is treatment? - Not very." "Are some approaches better? - No, all are equally ineffective." These, the authors say "are the easy answers, they continue to be passed down from one year to the next and appear in the most recent writing of some of the most respected professionals in the field". "But what support is there for these statements from scientific research? We believe that there is ample reason to question every one of these commonly accepted answers".

The review begins with a brief look at average outcome rates and spontaneous remission. These are described as two general reference points used in discussion of treatment effectiveness. Comparing Emerick (1974) and Costello et al (1977) they demonstrate that there are different ways of "cutting the cake" to obtain different

average outcome rates. Costello et al (1977) reviewed 80 studies reporting a 12 month follow up. Cases lost to follow up were counted as treatment failures, (such is generally found to be the case) and success was defined as the absence of problematic drinking. In this study success rates were found to vary between 12% and 45% with an average rate of 26%.

Emerick on the other hand followed the more usual procedure of ignoring cases lost to follow up. He, it will be recalled from our previous discussions, reviewed 265 studies and produced an average improved rate of 66% which was constituted by 32% abstinent and 34% improved. Others have produced abstinence rates for one year follow ups between 18% and 32%.

"An issue frequently raised regarding outcome is the lack of evaluation of non-drinking behaviours such as interpersonal and psychological functioning". The majority of studies reviewed in fact relied solely upon abstinence for outcome ratings although more recent research e.g., Sobell and Sobell (1978) has begun to incorporate broader spectrum variables. They make the point made elsewhere, that drinking behaviour does not necessarily correlate with other areas of life functioning. Drinking behaviour is regarded by Miller and Hester as a "minimal condition for successful treatment" - perhaps we should query this criteria. Whilst seeming at face value to be common sense it is nevertheless predicated on a number of assumptions about the primacy of consumption behaviour. If not, it involves a tautological definition, because it is already accepted that drinking behaviour does

not necessarily correlate with other sorts of functioning one would assess for outcome measures. In the absence of ideal reporting standards these two authors are in many cases obliged to evaluate studies according to their own terms of references.

Averaging the results of all the studies included in the review they produce an average spontaneous remission rate (abstinence plus improved) of 19% in untreated alcoholics at one year. This is much lower than Emerick's figure of 13% abstinence and 28% improved. Quite what it means to average figures in this fashion is not clear. We know differential outcome probabilities obtain for different groups of alcoholics and it might be wise therefore to keep rates desegregated for differing categories of drinking, particularly if these rates are to be used as the basis of comparison for treatment studies.

Their summary commenting on spontaneous remission advises caution because of the great variability in published figures, ranging as they do from 4% to 42%. This of course is not a new view and merely repeats the conventional wisdom. They make the interesting point that "spontaneous recovery within the group most comparable to treated subjects - those seeking treatment but randomly assigned to no treatment - may be much lower". This statement is based on only one reference in the review section Kissin et al (1970).

The rule of one third emerges in the average outcome rates at least for short term studies where one year follow-up data are used: "26% is the representative figure for

successful outcome (abstinent plus improved) 12 months after treatment."

Treatment Programmes

Psychotherapy :

Psychotherapy, in the United Kingdom as well as the United States, is a long and expensive process which is probably responsible for a very high drop-out-rate which in turn produces low average success rates compared to other methods of treatment. There is quite a lot of evidence in the literature, like the Maudsley study, which indicates that less intensive treatment, though not necessarily short term, is as effective as more intensive. Miller and Hester make the point that various authors like Blane (1977) have suggested that psychotherapy might be beneficial in reducing concurrent psychopathology and thereby improve the chances of maintaining therapeutic gains. This is said to be a testable hypothesis which has not yet been tested.

Alcoholics Anonymous:

The literature on Alcoholics Anonymous was reviewed. It is described as "the most popular and most widely acclaimed method of treating alcoholics. Uncontrolled studies suggest an abstinence rate of between 26% and 50% at one year, but controlled studies suggest its performance varies from no better than no treatment at all, to at least equally effective as other treatment. Bebbington (1976) suggested that a minimum effectiveness figure might be 26%. These reviewers claim that "in spite of a lack of supporting scientific evidence Alcoholics Anonymous has received widespread

uncritical acclaim". In Edinburgh Ritson (1968) found that prior exposure to Alcoholics Anonymous improved the prognosis of his clinic patients. Burt (1975) has suggested that the experience of Alcoholics Anonymous meetings and behaviour therapy have common elements.

Group Therapy:

Group treatment methods are said to be second only to Alcoholics Anonymous in popularity. Group therapy began to be used after the Second World War for a variety of patient groups. Like the generic term psychotherapy, a wide variety of procedures and theoretical orientations are subsumed under the rubric "group therapy". Doroff (1977) commented thus: "in recent years there appears to have emerged a consensus among the scientific and professional community to the effect that among the various psychotherapies a group approach seems to offer the brightest promise". Miller and Hester reply "whatever the basis for this confidence it is not to be found in the present treatment outcome literature". Among the reasons for this are methodological problems which make assessment of findings difficult. Also "Group techniques are seldom used alone, raising the familiar multiple treatment compound". The vast majority of studies reviewed were uncontrolled outcome reports, where group therapy had been only one element in the treatment programme. Improvement rates for group therapy have averaged around 40% for short term follow up.

Halfway Houses:

Halfway Houses arose in the 1950s to ease the transition from

inpatient to outpatient care. Research on their efficacy "has been characterised by methodological weaknesses that have clouded evaluation of other treatment approaches, particularly lack of adequate control groups and a large sample attrition at follow up. On the basis of present data there is no reason to believe that halfway houses significantly improve patient outcome in comparison to no treatment or to alternative treatment."

Family Therapy:

Family therapy presented Miller and Hester with a problem for their review as the great diversity of therapeutic and theoretical approaches made classification of this modality difficult for them. "In general, both controlled and uncontrolled treatment outcome studies have reported positive and adaptive changes in functioning within the family structure, in addition to abstinence rates ranging from about 45% to 80% at six months." They summarise family therapy thus: "Both behavioural family therapy and structured (systems) family therapy have received modest support from uncontrolled research. One controlled study found behavioural family therapy to be more effective than aversion therapies. Initial findings have been promising, but more controlled research is necessary before conclusions can be drawn."

Finally, Miller and Hester conclude this section of their review where abstinence orientated studies are discussed with a miscellaneous discussion of "other therapeutic approaches" - "Many other approaches..... have been tried, each with little or no empirical support at present". These

include, acupuncture, music therapy, art therapies and an extreme intervention was surgery on the hypothalamus of two alcoholics in 1973.

The section of the review dealing with treatment orientated towards abstinence was the largest and as the author described at the outset, most of the studies they reviewed were orientated toward total abstinence.

Controlled Drinking:

The next section deals with treatment methods orientated toward moderation. Much well designed, controlled research was done in the decade of the 1970s evaluating various treatment methods intended to produce what has come to be called "controlled drinking". I have already mentioned in this chapter the storms that broke with the publication of the Rand report and the acrimony involved. "What these initial emotional responses largely failed to discern is the difference between luring successfully abstinent alcoholics back to drinking and adopting moderation as a goal for certain problem drinkers at the outset of treatment". Controlled drinking outcomes had been routinely reported in early comprehensive studies such as that of Gerard and Saenger (1962). Pattison et al (1977) extended Emerick's list of studies to include those completed between 1971 and 1977 and listed 74 studies demonstrating controlled drinking outcomes. Approximately 12% of patients were controlled drinkers - but this includes a majority of studies which were evaluating abstinence orientated programmes. When only the 17 controlled drinking programmes are looked at in the Pattison et al review

the figure is 63% - that is 63% of the total population; 1,600 individuals from the 17 studies. Miller and Hester tell us that this figure of 63% is strongly influenced by three Japanese studies which comprise 83% of the subjects. When these Japanese studies are excluded the figure drops to 30% with an additional 11% abstaining.

Behavioural Self-Control Training:

(BSCT) has received a lot of attention in the literature and "represents an educationally orientated approach to the treatment of problem drinking. BSCT does not require any sort of gadgetry and can be done individually or in a group". It is also seen as having relevance for prevention programmes. The landmark study of Lovibond and Caddy (1970) mentioned earlier incorporated aspects of BSCT in the treatment package. These reviewers summarise their position with the comment: "BSCT appears to be a promising approach in teaching controlled drinking to problem drinkers. Remarkably consistent improvement rates of 60% to 80% have been found across diverse locations and also across very different treatment populations."

Referring to his own book, Miller and Munoz (1976), Miller says many of the components of BSCT are useful for self help programmes. The additional and, in view of previous research mentioned in this chapter, important point is made that BSCT does not require self labelling as "alcoholic" or lifelong commitment to abstinence.

Self Help:

Self help is to be distinguished from formal treatment at one

extreme and a social movement at the other. There have been numerous self help books aimed at total abstinence and a smaller though increasing number attempting to encourage and instruct people in moderate drinking. The Miller and Munoz book referred to above is called "How to control your drinking" and is a self help manual. From the mid 1970s Miller and his colleagues pioneered the use of self help manuals and the programmes based on them referred to as bibliotherapy. Miller and Hester comment that self help requires much more investigation, particularly so since like the process of spontaneous remission, there is much to be learnt from individuals who use bibliotherapy.

BAC Discrimination Training:

Blood Alcohol Concentration (BAC) discrimination training, "is a critical variable in alcohol research and treatment: it reflects the degree of intoxication and impact upon the body more accurately than do consumption data alone." As a treatment procedure, people are taught to make fine discriminations based on subtle internal cues between levels of BAC, such that eventually they can monitor their own BAC without feedback and use this skill when out drinking to identify safe cut off limits and stop drinking when these are reached. Lovibond and Caddy created a great deal of interest in BAC when they introduced "internal cue" training procedure as a component of their multimodal treatment programme. They had achieved impressive results with 86% improved at one year and 59% at two years and the results have also been replicated by the same researchers. There is clear evidence that people

can learn to discriminate BAC to maintain a steady (safe) level.

Miller and Hester conclude "all in all external cue BAC training appears to be a valuable component within multimodal treatment programs where the goal is controlled drinking, although it is far from clear if it is a necessary component of such programs."

Cognitive Therapies:

The reviewers make the valid historical point that with the rise of behavioural approaches to treatment, interest in cognitive issues had declined. "In recent years however, interest has been revived and the impact of the individual's cognitions upon her or his behaviour is being reassessed". Much of this renewed interest has been fostered by the work of Beck (1976). The therapeutic mix of cognitive methods with behavioural techniques is called Cognitive Behavioural Modification or therapy (CBM). The point is made that research on CBM is scarce generally. Two studies are referenced as support for this combination as opposed to cognitive or behavioural therapy individually.

Sanchez-Craig (1976) and her associates in Canada have produced a series of research reports describing methods for teaching coping strategies and generally adaptive behaviours within a framework of cognitive restructuring. At the time of writing the review there was only one controlled evaluation study available to reviewers: Brandsma et al (1980). Four conditions were applied: rational behaviour therapy, insight therapy, Alcoholics Anonymous and no

treatment. Followed up at one year all treatment groups were superior to no treatment. Both Rational Behaviour Therapy and insight therapy had lower attrition and better outcome than Alcoholics Anonymous but were both about equally effective.

Operant Conditioning:

In the early 1970s, behavioural psychologists began to investigate the influence of environmental contingencies on drinking behaviour of alcoholics. This research is generally called "operant" in that it examines environmental factors that operate to influence behaviour. Of particular interest are environmental contingencies that serve to reward or punish a particular response, in this case, drinking. Miller and Hester also indicate that during the 1970s a variety of in-patient programmes began giving alcoholics access to alcohol under control conditions for research purposes. Such research conducted in treatment settings demonstrated quite clearly that consumption was under the control of environmental reinforcement contingencies and that various other processes like modelling were influential. Amongst other similar studies reviewed were the Fixed Interval Drinking Decisions (FIDD) undertaken by Alterman et al (1978) and the Community Reinforcement Approach of Hunt and Azrin (1973).

Miller and Hester's overall conclusion then is that drinking is clearly amenable to modification by contingent reinforcement and punishment and that a variety of techniques have been developed. The problem they identify is that of translating them into the "real world".

The next type of treatment to receive attention in

this section of the review is multimodal programmes. It is in this report that Miller and Hester's review has been described as being of historic import in what might be called the modern or scientific phase of the alcoholism treatment literature. Miller and Hester tell us that the majority of the controlled drinking programmes reviewed have been of the multimodal sort. Some have impacted solely upon drinking behaviour, others have had a more widespread focus.

Nineteen studies are summarised, all of which have previously been discussed elsewhere in the present chapter. "Together, these studies provide a rather solid base of evaluation research. Of the 19 studies, 15 included and reported results from control or comparison groups, usually with random assignment to treatment conditions. Most included quantified assessment procedures and specified criteria for outcome ratings. In 14 studies, more than 90% of all cases were located at the longest follow up interval, and only one study located fewer than 80% (this study - Caddy et al, 1978 - finding 70% at three years post treatment). Perhaps most remarkable within the context of the alcohol outcome literature, 18 of the 19 studies checked client self reports against corroborative data sources. Few treatment approaches for problem drinkers have been subjected to the depth and breadth of research reflected in these studies".

The first study was published in 1970 the last in 1980. This sounds like a declaration that alcoholism treatment evaluation research has come of age, methodologically. Apart from approval for isolated studies,

there is no other roundly affirmative judgement by an experienced and internationally accredited authority, about the healthy state of research in this area. This is where its historical significance rests.

With regard to the empirical content of this section little by way of startling results emerged that has not been previously reported. There is in fact little evidence to support broad spectrum programmes for all clients. It follows that such intensive programmes may be valuable for some clients. Twelve of the studies compared broad and narrow spectrum approaches and most of the programmes used some variant of BSCT. The findings have been mixed.

The 19 studies were divided into two groups: those treated by BSCT "or treatment closely resembling it and those treated by more extensive multimodal packages". All subjects were given equal weight rather than simply averaging outcome percentages across studies with widely varying values of N. "Findings across these studies are sufficiently consistent that we believe this summary can be instructive". They parallel Emerick's data for controlled drinking studies outcome figures. Emerick found few controlled drinking outcomes in abstinence programmes: "it emerges here as the single most common outcome status." They make the very interesting observation that what they call "by-product abstinence" from controlled drinking programmes is as frequent as "by-product" controlled drinking outcomes from abstinence programmes as reported by Emerick (1975). The general finding that minimal treatment like bibliotherapy more or less

achieves what "more heroic interventions can", has, as the authors go on to say, implications for treatment planning. They conclude this part of the review with suggestions for a "levels" approach to treatment where everybody gets the minimum. Those for whom this is not enough get more. So the overall conclusion is that multimodal broad spectrum treatments are not cost effective and should only be provided for selective clients who demonstrate a need for it.

The final section of the treatment outcome part of this review deals with "teaching alternatives to problem drinking". Relaxation training has been a prominent procedure used in treatment programmes, though only relatively recently, the rationale being that people drink in order to relax and reduce anxiety, etc. Other procedures reviewed included Systematic Desensitisation which includes relaxation as an intrinsic component of the procedure (relaxation by itself is found to be of little value); Systematic Desensitisation has been found to be a useful adjunct to treatment. Social Skills Training has "received support in several controlled studies". They consider teaching alternative skills a logical but recent addition to treatment programmes and conclude that the volume of data does not allow firm conclusions as yet, but they are cautiously optimistic about the value of these procedures for treatment.

The final section of this review considers predictors of outcome. Miller and Hester suggest that an adequate review of this subject would take a chapter to itself, so long and extensive has the search for predictors of outcome been. The

reader is referred to four existing reviews (Baekeland 1977; Brandsma et al 1980; Armour et al 1978; Gibbs and Flanagan 1977) all previously referenced in this chapter) "Suffice it is to say that no consistent individual differences have been found to predict outcome, although most frequently mentioned are variables related to social status and stability (e.g., work history, marital status, socio-economic status)". They make the point that the practitioner would benefit most from a knowledge of differential response to alternative treatment strategies. This of course relates to the issue of matching discussed previously. Cronkite and Moos (1978) are cited for their important research which found that "interactions between patient variables and treatment variables accounted for 23 to 40% of the variance in treatment outcome" and suggested that increased attention be paid to matching of patients with appropriate treatments.

Discussion of predictor variables in this section is restricted to:

- A) the value of varying amount of treatment contact
- B) the efficacy of various types of therapists and
- C) criteria for differential assignments to abstinence versus moderation goals in treatment.

Miller and Hester conclude that controlled studies have "very consistently found that extensive and long term programs, whether orientated toward abstinence or toward moderation, are no more effective than briefer and less extensive approaches." It appears that some chronic alcoholics may benefit from more extensive interventions. Differential

diagnostic procedures are advocated. They comment that extensive treatment might not only be unnecessary but "perhaps detrimental for some patients".

Type of Therapist:

Who is and who is not qualified to treat the alcoholic has been the subject of heated debate for some years, according to Miller and Hester who reference a paper by Krystal and Moore dated 1965, called "Who is Qualified to Treat the Alcoholic: a Discussion." They discuss the much referred to paper of Kalb and Propper and regard as "improper" their contention that the craft and science dichotomy is an either/or matter. In support of this they ask us to consider Krystal's (1963) "contention unburdened by the weight of data", that "the therapist for patients addicted to self-medication with alcohol must be able to do effective psychotherapy."

Paraprofessionals, they say, hold no monopoly on dogma. They conclude that "present data do not indicate substantial differences in the effectiveness of professional versus paraprofessional therapists or of alcoholic versus non-alcoholic counsellors." However having concluded this from the data, Miller and Hester go on to argue that the emergence of new alternative treatment methods will require a prescriptive approach in which the professional is very much in control of not only producing the more technically complicated sorts of treatment but very definitely controlling the total treatment programme as well as evaluating it. "In this way optimal use is made of staff time, with each team member performing therapeutic services appropriate to his or

her level of training and expertise."

After discussing therapist characteristics Miller and Hester focus on predictor variables. Predictor variables raise the issue of criteria for choosing between abstinence and moderation as a treatment goal. They tell us "the picture that emerges is clear: individuals who will become successful controlled drinkers show less resemblance to the classic diagnostic picture of alcoholism, they have fewer problems related to drinking and have had them for a shorter period of time, have fewer symptoms and family history of alcoholism, and drink less - they are more likely to be women, to be younger and not regard themselves as alcoholics". Social stability increases the probability of a successful outcome of both sorts. Miller and Hester conclude that controlled drinking programmes are for those individuals who are at "earlier stages of the development of problem drinking".

Review Conclusion At the beginning of their conclusion and discussion section Miller and Hester comment that the literature on treatment effectiveness has been growing at a rapid pace and that they are "particularly encouraged to note an increase in recent years, in the number of well designed studies and in the use of adequate assessment procedures." "...the field of alcohol treatment is at last accumulating a usable body of knowledge regarding treatment outcome. We are, happily, well past the point where all treatment approaches can be regarded as equally valid and effective." These highly affirmative statements may cause some surprise, so accustomed have we become to criticism through our previous encounter with the literature considered in this chapter, the negative appraisals of methodology and the ubiquitous finding of the "rule of one third".

What exactly have we learnt from this review other than the fact that it has been possible to make historical statements about methodology? In a nutshell, what is clinically usable information? They say "first of all, it is clear that certain methods are not supported by research to date..... they are ineffective , uneconomical, or unjustifiably hazardous for problem drinkers". Included here are drugs, insight psychotherapy and routine multimodal treatment, although they do say it remains to be seen if any of these approaches have any value for particular subgroups of clients - "The majority of treatment procedures for problem drinkers warrant a scotch verdict of not proven at the present time". Ironically, the most "widely accepted and commonly used

treatment techniques currently fall into the category of unproved, largely due to a lack of appropriate evaluation research". This much we know from routine reports in the literature. What about the usable knowledge as to what techniques to include as opposed to exclude from programmes? Although the language becomes somewhat tempered, not quite what the phrase "valid and effective" had implied and led us to expect, we are told the efficacy of certain procedures is tentatively supported by research existence.

These procedures are disulfiram, electrical aversion, videotape, self-confrontation and chemical (nausea) aversion. These are strategies whose efficacy is only bought at the cost of some risk, or discomfort "which might outweigh the attendant benefits". Behavioural self-control training and "broad spectrum approaches including desensitisation and social skills training have received sufficiently consistent support from outcome studies to be considered as tentatively supported". With these we are told there are no unwanted side effects. These are very welcome assertions; although only tentative recommendations are made, at least we have clinically useful information as to what to include. However, the carpet is not entirely pulled out from under one's feet, is to say the least, given a fearsome jerk with the very next sentence. "Nevertheless, no treatment method has been shown to be consistently superior to the absence of treatment or to alternative treatments in a sufficient number of well-controlled studies to warrant 'established' status." So in a sense the big questions are still to be answered; nevertheless

in betting terminology we are given some procedures to put our money on. As the reviewers go on to say we have learned much about what does not work, "but there is still much to be learned."

With regard to what to do with clients we are on somewhat firmer ground. "Predictive data, for example, suggests that for early stage problem drinkers, moderation-orientated methods may be optimal, whereas more advanced alcoholics may be best served by effective abstinence-orientated approaches." There appears to be good evidence that intensive and extensive treatment confers no advantage over minimal treatment, at least for many clients, and therefore more intensive or long term contact should only be provided for those who do not respond satisfactorily to minimal treatment. In programmatic terms, this implies a comprehensive treatment setting arranged on a phase level basis.

One could comment facetiously on the continuing failure to identify more definitely treatments which can clearly be shown to have the edge over no treatment. To do this is no easy task and involves much more than merely a consideration of what treatment is. Again we are faced with large issues about the nature of the beast and what exactly is entailed in recovery. The claims made in the review about the refinement of evaluative methods and the valid accumulation of knowledge of a clinically useful sort are clearly valid. But we must balance our judgement by bearing some of the big questions in mind. For instance, could it be the case, that

validity achieved through ever more refined and scientifically sound research procedures has had the consequence of limiting the scope of our enquiry. I will touch upon this again but for the present I would like to close this discussion of research undertaken in the 1970s with a consideration of a paper of a different kind which might help us contextualize some of the research reported here and set the context for my final section.

The paper I wish to discuss is by Robin Room and was originally prepared for a symposium on research priorities at Rutgers Centre of Alcohol Studies in 1977 (Room 1978). Room started his article with the comment that in each of the social science disciplines significant work had been done on alcohol issues by the early years of the century, but that in recent years (e.g. the 1970s) there had been an exponential growth in such work. Even in 1961 it had been possible to write a "synoptic" review of such work within the confines of a single paper. Such would not now be conceivable. He adds that recent years have seen a modest resurgence of interest in economic, political science and historical studies of alcohol issues. These together with anthropology, sociology and social psychology - the disciplines he had in mind as doing the the original work - make a very fair representation of social science endeavour.

He describes in some detail the diversity of the work engaged in by social scientists in the alcohol field - and extremely diverse and extensive it is. Yet despite all this work in all its "diversity, depth and frequent

excellence..... it has a curious lack of presence, both in alcohol studies in general and in social science in general". He makes the observation that alcohol related articles are scarce in major general sociological, psychological and anthropological journals. This he thinks can only be explained in part by the prejudice of academic social scientists against applied social problems research. Indeed drinking studies have a low rating among social problems topics - "Social scientists working in alcohol studies have failed so far to communicate to their fellow social scientists the strategic usefulness of alcohol phenomena as a focus for analysis of many social issues".

This is worth considering. I think Room is undoubtedly correct and many of the issues that have already arisen in this chapter about the failure of research to articulate theoretically the interactional and social environmental determinants of drinking careers and recovery would seem to substantiate Room's point about social sciences neglect of alcohol studies. In an important sense, this thesis is a small scale attempt to introduce a social science concept into analytic work in the field.

In a more general mode Room comments upon social science research and the Alcoholism Movement. There has, he argues, been a "clash of perspectives" between these two. The Alcoholism Movement has for forty years been a close amalgam of interests united by commitment to a conceptualisation of alcohol-related problems in terms of a single entity, nowadays usually identified as Alcoholism or Alcohol Dependence. This

movement has been in control of alcohol studies since the 1940s and continues to be so in the United States although the position might be conceptualised slightly differently in the United Kingdom. Room makes the point that a lot of social science research subsumed under the rubric "The effect of alcohol on society" - a characterisation prescribed by Jellinek - was only useful as a public relations gesture to obtain funds for other sorts of research - primarily the individual courses of inebrity. The clash is very evident here - "the strain tends to occur over how the dependent variable is to be defined. In the era of the Alcoholism Movement, it has been alcoholism defined as a clinical entity, that is to be explained. So long as social scientists accepted this definition of the dependent variable uncritically, and investigated its social epidemiology, their efforts were welcomed and heralded". This is the source of the argument that social science's contribution to the alcoholism literature is on cultural differences on rates of alcoholism. But some social scientists, for example Room, have not only pursued their own interest. American sociologists in particular, and, of late, some British psychologists have not only studied alternative dependent variables, but have also offered their services in reconceptualizing the Alcoholism Movement's dependent variable. This offer has been unwelcome or at least unheeded by others in the alcoholism field.

This reconceptualisation has begun to proceed in a number of directions, Room instances the recognition of

multiple discrete problems in the general population from surveys of drinking problems. Social scientists have also been responsible for the nominalistic critiques of alcoholism status as a disease entity. These and other social science projects are a direct threat to the hegemony of Alcoholism Movement thought.

Another reconceptualisation which has occurred since Jellinek's formulation of the core problem in 1943 has been the reversal of the dependent variable. Alcoholism becomes part of the explanation, rather than what is to be explained. Because of this switch, Room explains, social scientists and others have started to talk about "problem drinking" rather than alcoholism. However, such studies, Room suggests, are seen as being somewhat "beside the point in alcoholism literature". They are not about the "nature and etiology of alcoholism".

A further instance of divergence cited by Room concerns the social study of normal behaviour: normal drinking is studied the better to understand abnormal drinking. Against this perspective the disease theory insists upon two discrete groups: normal and alcoholic. We have a major schism here. Room comments that studies of normal drinking have been seen as "ornamental scholarship, irrelevant to the main tasks of alcoholism research."

A final divergence mentioned by Room is the propensity of the social scientist to view social action as interaction, i.e. interaction between drinking behaviour and social reaction to such behaviour. Both formal and informal

socially responsive behaviour becomes an important variable for the social scientist.

Speaking in 1977 Room was not altogether pessimistic for the prospects of social science having an impact on alcohol studies. There are, he says, "signs of change. We seem to be entering a post addiction era in alcohol studies where no one paradigm dominates thought and research." With the benefit of hindsight we can say that the social sciences research effort might have had a more receptive academic climate, but certainly not an economic one. (Room could not have foreseen the advent of the Reagan era which by popular account has left social science research in the United States severely handicapped).

Room lists a number of general topical domains of social science research which will be of value for alcohol studies, like normative and ecological research on drinking behaviour and problems, studies of drinking careers and natural history of drinking problems; and importantly in my view, studies of formal and informal treatment processes and community responses to alcohol problems and alcohol control policies.

Also in a more optimistic frame, like Miller and Hester, Room acknowledges a number of desirable methodological trends:-

- 1) Disaggregation, or decomposing the dependent variable into different types of drinking behaviour and consequences.
- 2) Convergent methodologies - Room makes the somewhat exaggerated claim, in my view, that there has been a "welcome

tendency to subordinate methodology to content, rather than organise studies and research traditions around methodological positions; often the use of various methods can provide emergent validation".

3) Time and history - increasing attention is being paid to the historical dimension of alcohol studies and to time series analysis, the macro and micro levels of analysis.

4) Change and natural experiments - coupled with an increasing emphasis on time has come the study of substantial changes in populations, such that they are viewed as natural experiments.

5) Policy relevance - Social science has become more helpful to policy considerations as one aspect of its endeavour, but as Room indicates "greater policy relevance may imply ideological constraints", and almost certainly does.

Room makes a final point about poor organisation of alcohol social science research in the United States, a state of affairs which exists in the United Kingdom as well. Although Room speaks of social science and social scientists the disciplines he has in mind are social psychology of a non-experimental sort, anthropology, history and preeminently sociology. However the two occupational groups most in evidence in the literature have been clinical psychology and psychiatry.

Two significant developments during the 1970s should be noted. During this decade psychologists in large numbers began to espouse a reconceptualisation of classical and operant conditioning in terms of Bandura's social learning

theory (Bandura 1977) and the development of cognitive behaviour theory, based on social learning theory (Beck 1976).

Psychiatry has been firmly associated with a major theoretical production of the World Health Organisation in the mid 1970s (Edward et al 1977). This was the World Health Organisation publication: "Alcohol Related Disabilities" which includes a formulation of the somewhat contentious concept of the Alcohol Dependence Syndrome. The Alcohol Dependence Syndrome has been the centre of controversy mainly between social scientists, particularly psychologists and psychiatry. In brief the Alcohol Dependence Syndrome has been seen as a disguised form of the disease theory. This is usually translated into political terms as an attempt by psychiatry to dominate the field of alcoholism treatment. (For a description of the syndrome see Edwards and Gross (1976), Edwards (1977), Hodgson et al (1978) and for a critique of this literature see Shaw (1979) with a reply by Hodgson (1980).)

To summarise, there was an enormously important and extremely prolific output in the decade and this resulted in a number of important developments. Alcoholism treatment evaluation studies presented a dismal picture of alcoholism treatment not dissimilar to that in psychotherapy. The pessimism reached its nadir around the mid 1970s when, after the Rand and Maudsley studies it was widely assumed that treatment of whatever sort did not work. At about the same time and no doubt partly because of the widespread nihilism,

some enlightened clinicians and researchers began to think about curative factors in a person's environment. We have seen how the very discipline which might have pursued interesting and productive research questions about social interactions and general sociological issues concerned with recovery was considered to be "ornamental scholarship". The presence of clinical psychology as an increasing force in alcohol studies has been a feature of the field which has had a major impact on treatment and its evaluation by the mid 1970s. Psychologists were challenging psychiatrists for academic leadership of the field and were by then certainly leading the field in terms of research output on treatment. This development had accelerated from the mid 1960s when psychologists first introduced behavioural analysis of drinking behaviour. They imposed behavioural methods and a preference for strict scientific procedures. I have noted that the scientific emphasis was extremely powerful, which I have suggested entailed the danger of producing a blind faith in methods at the cost of imagination. This sort of belief which I called scientism is an observable theme in the literature, at least amongst some writers. Generally the empirical or bottom-up data-led developmental preference of many research psychologists as opposed to the top down or theory driven approach, which would have been better able to raise new and more novel research questions, has been most in evidence.

It has been suggested that sociologists who might have been better placed to pursue a theory driven perspective

were undervalued, such that we end up with lots of data and not much theory to make sense of it. Also in the mid 1970s most of the research had a strong individualistic bias. However behaviourism and primitive behaviour therapy waned and from the mid 70s to the end of the decade, new theoretical bearings in the shape of Social Learning Theory were in evidence as well as new treatment methods based on them. In theoretical terms there was by the end of the decade enthusiasm for a theory that could encompass everything behaviourism could as well as the mediating role of cognition. Hence a much wider range of explanatory variables could be considered, including social environmental ones in interaction with person variables. A new set of cognitive behavioural treatment methods was increasingly being used with enthusiasm, although these were still in their infancy so that no hard predictions about them were possible. If one takes a systems view of the total alcohol treatment field, it can be asserted that there was a great deal more variety in evidence by 1980 than there had been a decade previously. We have touched upon some of this variety in this review so far, the most salient aspects of which were, in my view, the shift from a unidimensional to a multidimensional view of problem drinking, its treatment and treatment outcome evaluation and the increased theoretical and methodological options. One can even construe the rancour over the notion of the Alcohol Dependence Syndrome as a piece of growth promoting system deviance leading to an increased range of research questions.

I propose to bring this review chapter up to date by paying attention to methodological issues and general debates that impact upon the study of alcoholism treatment outcome. To set the scene, as it were, I would like to introduce a paper by Nick Heather which fulfils a similar function to the Room paper with which I concluded the last section.

In contrasting tone to the closing comments I made above, Heather (1980) starts his paper by saying the alcoholism treatment world is in upheaval. "Established certitudes about the nature of alcoholism have been undermined, conventional treatment practices challenged in a fundamental way, and indeed, the most basic assumptions which have traditionally shaped our understanding of alcoholism have been seriously called into question. It is not even sure whether "alcoholism" exists any longer. No wonder some people are asking "Does anything work?" He sets out to explain how this confused state of affairs has arisen and nominates a single cause - "the decline of the disease theory of alcoholism" which has come about in the wake of research evidence relating to the issue of "controlled drinking" in former alcoholics. He then goes on to explore the implication of this evidence for theory and practice in the field of alcoholism treatment. The turmoil and polarisation in the field (i.e. for and against the disease model in the United Kingdom and the alcoholism movement against the rest in the United States) is explained by likening events in the field to

a Kuhnian paradigm change where the anomalous finding of successful controlled drinking in former alcoholics cannot be accommodated by the status quo of normal science, so the paradigm breaks down, typically, "amid considerable bitterness and controversy". A new paradigm arises out of the smouldering resentments of the old with a whole new agenda of "theoretical problems, evidential criteria and unique research methods."

That is one possible Kuhnian view of events. Another is offered with the same consequences. The old paradigm of alcoholism as disease might best be seen as being a period of prescientific knowledge, a "folk science" out of which a new paradigm emerges. The first shot which precipitated the scientific revolution was of course Davies paper of 1962 already discussed. Other sorts of evidence hastened the demise of the old theory. Heather mentions some, including the finding "that the distribution of alcohol consumption in a population is continuous and unimodal and not bimodal as would be predicted if a discrete sub population of alcoholics were to exist" (De Lint 1976). He refers to the most recent version of the disease conception of Alcoholism, The Alcohol Dependence Syndrome (Edwards and Gross 1976) as trying to avoid the contradictions by abandoning the postulates of a distinct entity "although it is possible to argue that the assumption of a disease entity has been retained in disguised form". Heather then goes on to instance contrary evidence for another corner stone of the disease theory: "progressive deteriorations of symptoms". This of course is what logically

prohibits controlled drinking.

Turning to the wider political issues Heather remarks that it is not the prerogative of the medical profession to label social deviance. He makes the telling statement that "in other areas of criminal, sexual and political deviance it is non-medical opinion which has been largely responsible for stemming the tide of medical imperialism and for placing the debate about the nature of this deviance, and society's response to it, in the arena of psychological, sociological and political science where it belongs." He asserts that the same process is now at work in the realm of alcoholism. This assertion gives a clear view of the competing strains at work.

Returning to empirical issues again he gives a succinct statement of the state of play as seen from the perspective of the anti-disease lobby. "The phenomenon known as alcoholism does not betoken an irreversible disease but a reversible behavioural disorder". The issue of reversibility, he instructs us, is the crucial issue by which the disease notion stands or falls. He then asks the crucial question: "What then is the new paradigm for alcoholism?" "It is", he says, "transparently clear. A theory grounded in learning theory, which entails the assumption that drinking behaviour of alcoholics is in principle, modifiable." He points out that modifiable does not necessarily mean modifiable by treatment and the evidence on spontaneous remission is one of the main sources of the new paradigm. "A further fundamental assumption of the new paradigm is that there is no essential qualitative discontinuity between the drinking behaviour of

persons labelled as alcoholics and drinking behaviour in general." The learning theory will not be of the naive behaviourist sort but will in fact be social learning theory which will demand that the new paradigm is supported by the use of a sound scientific method for its research, employing a sociopsychological learning conception of alcohol use and abuse.

This then is the new paradigm for problem drinking and one that finds a wide consensus. Hitherto, abstinence was the treatment goal of choice for the majority of people and controlled drinking was only justified in particular cases - as an exception to the general rule. The usual exception was that such particular cases "were not 'real' alcoholics". Heather argues that this state of affairs should be reversed. However this, he points out, is a theoretical recommendation "primarily concerned with research developments". Since the theory is incomplete, treatment must proceed along pragmatic lines, but it must be responsive to research findings and eventually links must be formed between treatment and theory.

There is of course a logical absoluteness about abstinence. One cannot have drinking problems if one does not drink. However we have seen from evidence reviewed that people generally do not abstain regardless of what treatment attempts to prescribe. Heather writes that "there are some who argue that treatment does not work" and cites Emerick and Orford in support, but says a "close" reading of these authors leads to a contrary conclusion. Emerick's work in fact shows that more intensive treatment resulted in more improvement and

Orford's work showed that more severely dependent drinkers benefited more from intensive than minimal treatment.

Controlled drinking treatments will have a number of advantages, argues Heather, including reducing dropout and increasing referrals. Because controlled drinking treatment "straddles the border between treatment and education" many more people with less severe problems could be reached. There is, he says, at the moment a huge gap between "simple written advice given on posters and leaflets and that of the potentially stigmatic panoply of treatment services". The bibliotherapeutic experiments of Miller and his colleagues as well as Heather himself and his colleagues in Scotland offer a glimpse of such 'community education' initiatives which are a natural consequence of a learning model of alcohol problems and treatment.

Commenting upon the provision of controlled drinking treatments as of 1980, on the basis of a survey of United Kingdom agencies he and his colleague Ian Robertson undertook, Heather informs us that although many treatment agencies offer a controlled drinking treatment, it tends to be offered only to the special cases of the sort he has already mentioned. "In other words, the abstinence ideology continues to pervade the alcoholism treatment services in the absence of theoretical or empirical support".

Two important issues emerge from Heather's discussion of empirical evidence and theoretical trends which were emerging in the alcoholism field and which bear strongly upon treatment and its evaluations. The first is the potential of

cognitive social learning theory as a theoretical basis for making sense of a vast amount of data, such that the new paradigm can be discerned. The other concerns the old issue of matching, given that, theoretically everyone's behaviour is modifiable.

Optimal matching of patients with treatment has a common sense appeal and has become an issue which is very much on the agenda in treatment evaluation. However, little as yet exists in the way of validated matching procedures that are clinically useful. Indeed matching is still very much at the stage of requiring further conceptual clarification and this has been the focus of a paper by Finney and Moss (1986) in which the necessary parameters for satisfactory matching are discussed in the context of what has actually been done. They take the view that much of the matching so far carried out has been based "on methodological assumptions that are not commensurate with the complexity of the matching problem".

They start their discussion by noting a rather recent enthusiasm in the field for a prescriptive treatment principle, although they tell us such research has occurred spasmodically on this issue for "40-50 years". The point is made that work in the field of alcoholism treatment evaluation which has looked for main effects from experimental and correlational studies has been disappointing. The attempt to investigate outcome by correlating specific patient intake characteristics with outcome has also not provided useful indicators, with the possible exception of "social stability" as we have seen in the Gibbs and Flanagan (1977) review.

Experimentally designed evaluations, where patients have been randomly assigned to various treatments, likewise tend not to give encouraging results. They tend to be open to alternate explanation about method variables. Studies of pretreatment characteristics of patients typically account for 20% or less of outcome variances.

Finney and Moos list 4 basic components of the matching hypothesis - patients - treatments - outcomes - interaction effect. The first three are conceptual in nature. The initial task is to select the effective matching variables from a vast array of patient and treatment characteristics, nominate the desired outcomes that matching is to improve upon and then determine at what stage in the treatment process the matching will be effected. Then there are methodological issues which arise from this which are:

- A) non-linear interaction effects,
- B) higher order interaction effects and
- C) multilevel interaction effects.

Patient variables are conceptualised in three categories:-

- 1) A deficit resource dimension. There are innumerable variables to chose from i.e., heavy drinking, occupational problems, ego strength etc..
- 2) Information processing variables i.e., cognitive abilities or deficits.
- 3) Environments i.e., life contexts - chronic strains - social supports etc.

Treatment variables are said to be just as numerous

as patient characteristics and many of those which apply to clients apply equally well to treatment staff. Two broad classes are noted:-

- A) Therapeutic Components i.e., treatment methods globally described, behaviour therapy, drug therapy to specific techniques employed such as confrontation etc.
- B) Treatment Delivery Process. The presentation of programme i.e., its phases, repetition and duration - issues of quality and also treatment provision characteristics.

Finney and Moos provide a helpful tabulation of these variables as follows:

PATIENT	TREATMENT
Deficits - Resources	Therapeutic Dimensions
Person - Environmental	
Information Processing	
Capabilities	Delivery Process

When it comes to selecting patient variables from these two categories any one of a number of strategies might be employed. Finney and Moos feel that clinical judgement is usually used to match the patient to the treatment. Other methods might be the "cafeteria approach" (i.e. let the patient choose), exploratory data analysis, data reduction techniques and theoretical analysis. As yet there would appear to be no generally approved method. It is known that clinical judgement is not too reliable. With regard to the patient choosing, they cite evidence to the effect that treatment effectiveness and treatment attractiveness are not necessarily related.

The other approaches are more technical and systematic. As previously discussed Smart (1978) found no

significant results when he explored a large data set that produced 186 first order interactions, as such explanatory data analysis has an unpromising track record. Data reduction methods, where a large number of variables are reduced to a few general dimensions usually by some variety of factor analytic technique where the factors may be purely empirical, can be used. Or confirmatory factor analysis can be employed, the selection of variables being guided by theory. Data reduction methods are of special relevance to the main theme of this thesis - Affect Balance - as well as matching. Finney and Moos explain that there are variable examples of the data reduction approach to exploring patient-treatment interaction in the alcoholism literature.

A fifth method relies upon the theory to guide the selection of patient and treatment variables. An interesting example is provided where patients were graded in terms of their conceptual ability against treatment structure. The matched sample did twice as well as the mismatched sample at follow up. In discussing this issue they instance a widely quoted paper by Skinner - "Different Strokes for Different Folks" (1981), where the efficacy of experimental designs with random assignments of patients to treatment is argued for. The interesting point is made that it is not so much the statistical power of the experimental studies that produced results, but rather that the experimental studies were "generally driven by theories, whereas the non-experimental generally were not", and also that experimental studies generally used more distinctive treatment modalities.

Formulating "powerful theories seems more important than methodology as the primary key to developing effective matching schemes". In the main body of the paper Finney and Moos assert that theory development is the most pressing need for the issue of matching. Although they advocate theoretical matching as far as this is possible, they see exploratory data analysis as a way of triggering "fruitful theoretical insight".

A discussion of methodological issues that bear upon conceptualisations of effective prescriptive treatment is presented.

Treatment advances, argue Finney and Moos, are likely to be small and incremental. As knowledge of patient treatment matches accumulates it should be possible to develop more elaborate approaches that mirror the complexity of the task.

Two procedures are considered by Finney and Moos to be of special importance at the present state of knowledge: consideration of the individual information processing capabilities in respect of type of treatment provided, and attention to matching of patient extra-treatment environments to treatment received (Moos et al 1980).

They conclude their paper with a summary statement about the complexity of the matching task: "patient-treatment matching may involve multiple patient (personal and environmental) and treatment (therapeutic and delivery) dimensions interacting in multiple forms in "Powers" (linear and non-linear), in multiple levels and during multiple stages

of the treatment process to influence multiple end results and outcomes". However this daunting statement is balanced with a comment on an extremely helpful paper by Weick (1984) entitled "Small Wins: Redefining the Scale of Social Problems".

Essentially Finney and Moos advocate the adoption of a "Small Wins" strategy where complex problems are scaled down (or perhaps scaled up) such that the targeted small win becomes a more manageable task and the researcher is less likely to be psychologically "immobilised" by the overall problem.

My rationale for considering the Finney and Moos paper at this stage before I have actually mentioned any treatment evaluation reviews undertaken since 1980, is that we need to bring a new awareness of the issues involved to make sense of outcome research in the 1980s. Heather and Moos both illustrate the crucial importance of matching under the new paradigm.

However at this point something of a contradiction appears. The kinds of recovery uncovered by community surveys and studied by Tuckfeld tend to leave an impression of a rather less specific or more global process than that which appears to be emerging from treatment settings, which might be described as an incremental model. However I suspect that the difference is again due to a possible failure to distinguish between theory and method. Incrementalism is of course in keeping with the methods of behavioural psychology and is if anything further implicated in more recent cognitive behaviour therapy which is the other major development described by Heather. Recovery in the community without the aid of formal

treatment and predicted by global indicators like social stability, age and marital status etc. require explanatory theories which deal with social processes. Such sociological theories have been scarce by comparison with psychological theories that in addition have a very definite reductionist core. The actual methods that have had the most success to date have of course been reductionist in practice and hence they bolster similar theories. However as Moos has suggested with matching, theory driven approaches are probably most efficient. Contradictions between the incremental view of treatment and the social process or extra-treatment effects may well be caused by the absence of a social science theory of the latter. To some extent I hope the concept of Affect Balance will contribute to such a theory.

To continue with a methodological review and overview of treatment effectiveness, Emerick (1983) asks what are we to make of contrasting findings like those of the Rand researchers who found only 7% abstinence at follow up and Miller and Hester who produced a figure of recovered and improved of 26%. For instance, variability of the order of 7% to 90% has been reported. Does such variability relate in any meaningful way to actual treatment provided or to other non treatment variables? Emerick and Hansen construct their review of issues that influence internal and external validity round these two figures, the low one from the Rand study, the high one from an advert in a National Alcoholism Magazine for a hospital programme in Georgia, plus the fact that less than 10% of alcohol dependent people ever receive formal treatment.

They demonstrate that insufficient attention is paid to methodological issues such that validity can not be supported by results in the fantasy region of 90% whereas the Rand figure of 7% is more factual.

Moos and Finney (1983) comment optimistically on the development of "a conceptually based approach to treatment evaluation", which has developed "out of current trends in behavioural medicine and evaluation research". Such an approach can help integrate seemingly disparate facts about alcohol abuse. In short, "evaluation research is beginning to fulfil some of the promise it derives from its unique location at the interface of basic research on alcoholism and applied concerns with the development and delivery of treatment programs".

Investigators who adopt a person-centred or disease model and attempt to predict outcome from predisposing factors "typically find these factors account for only a small proportion of variance in drinking and drinking-related outcome criteria" (usually less than 20%). The facts that people mature out of alcohol problems and others obtain intermittent improvement tends to support the view that alcohol abuse can be treated successfully.

In summarising the evidence, they assert "the beneficial influence from treatment and extra-treatment contexts can help some alcohol abusers to resume essentially normal lives. Formal treatment is neither necessary nor sufficient to effect long term improvement. However treatment facilitates the recovery process in that treated individuals

show higher rates of improvement in many studies than do minimally treated or untreated comparison groups." This appears to be quite a definite position statement.

Lurking about in the literature has been a suspicion, not frequently articulated, that treatment in fact is counteractive to normal curative influences in "the environment". It depends of course on what sort of treatment is being considered. But generally we have here a strongly affirmative statement about treatment that accounts for extra treatment factors. This is not to deny that some treatment programmes do not do very well with some individuals. Attrition and relapse rates are very high, and of course many individuals do not spontaneously recover. All these conflicting sorts of evidence indicate that both treatment and life context factors can have a powerful impact on the course of alcoholism. These are the issues that are beginning to be addressed by the new conceptually based approach to study how patient, intervention and life context factors interact to "effect recovery and relapse".

This new approach to evaluation research is called a Process Orientated Model for Alcoholism Evaluation Research. This model replaces an "idealised paradigm" described as "summative" where clients are assessed, assigned and reassessed. Two sorts of realisation have been important in the development of the new model. One is that researchers have concluded that treatment is not a static variable nor is it always implemented as planned. The other is a growing awareness of the importance of extra-treatment factors. The

study of treatment process variables and life context variables in relation to outcome, expands the scope of evaluation from a purely technical activity as in the summative model such that it can help formulate conceptual issues, and it "produces its greatest yield when it is grounded in a conceptual framework".

This model places much greater emphasis than has previously been the case on integrating the various components and emphasising the detail of treatment delivered as well as extra-treatment factors. Illustrating the necessity of a thorough treatment implementation analysis Moos and Finney cite a study by Malcolm et al (1974) where 31 disulfiram implants were examined. One week after implantation only 8 were found to be positive in blood samples. They comment "what could be more direct treatment than disulfiram when it is subcutaneously implanted?" The linking of process analysis to outcome involves setting one programme against another and introducing or removing an element of treatment. Usually this sort of process analysis has used meta analysis. Moos and Finney use the example of a series of studies by Costello (Costello 1975; Costello et al 1980). Costello reviewed outcome in 58 studies that used a two year follow up. From these he produced a profile of the elements of the most effective programmes. These were:

- A) an active intensive milieu
- B) moderate length of inpatient stay (6-8 weeks)
- C) Disulfiram
- D) behavioural therapy

E) aftercare

F) involvement of relatives and employers.

These "strong components" were incorporated into an ideal inpatient programme. One and two year follow ups of these intensive inpatient programmes, yielded a success rate of 40%. This was on par with the best results for similar populations.

Another aspect of process analysis concerns the quality of treatment where the client-therapist relationship has been the focus of research. Miller et al (1980) demonstrated that the degree of therapist empathy was predictive of outcome. At the programme or milieu level, Cronkite and Moos (1978) showed that the clients' perception of the entire programme was important for outcome at 6 months relative to other client and programme variables. The judgements were made on the Outpatient Treatment Environment Scale dimension of programme Cohesiveness, and by good Organisation of a programme orientated toward Independence and Personal Autonomy. It is of some interest in specifying the terms for inclusion in this treatment process component of the model that the treatment component and the social climate of the programme have independent effects on outcome.

What is especially interesting in this model is the specification of extra-treatment factors. Moos and Finney list only three components under the rubric "life context factors" namely family, work and life stressors. However these are convenient but not exclusive categories. They make the important point that this new paradigm assumes that

treatment is part of an "open system". Treatment is only one component (indeed only a temporary one) of the "multiple environmental microsystems, or specific settings in which a client is involved". During treatment, and even more directly afterwards, a client is exposed to a myriad of influences emanating from other, more enduring microsystems such as family and work environments. Evidence of the relationship of family settings where the milieu is cohesive and supportive, to successful outcome is briefly reviewed. They note in addition that this research indicates that family treatment should be more widely provided. They also note studies that bear on the relationship between an individuals' work environment and outcome. The situation is very similar to that of the family. Where the work setting was more cohesive and involving superiors who were supportive, a better outcome is predicted.

Life stresses have been studied in the context of relapse. For instance Marlatt and Gordon (1980) showed that relapse was more frequent within the first 90 days after treatment. Social pressure to drink and interpersonal conflicts were centrally implicated in the relapses noted by Marlatt and Gordon. This work by Marlatt and his co-workers led to the development of a whole research programme and the development of a model of Relapse Prevention which is of considerable importance for alcoholism treatment in the 1980s. (See Marlatt and George 1984 and Marlatt and Gordon 1985 for a full description of Relapse Prevention).

Moos cites his own work to illustrate the

relationship between negative life events and poor outcome (Moos et al 1981). Although the study of extra-treatment factors is still a comparatively little documented area in the outcome literature, a study by Cronkite and Moos 1980 demonstrated that the inclusion of extra-treatment variables in the model more than doubled the explained variance in treatment outcome.

Another treatment recommendation, made by Moos and Finney on the basis of this evidence, is that "alcoholism treatment may be more effective when orientated toward patients' ongoing life circumstances". Moos and Finney suggest that there is a continuity in the data between treatment and extra-treatment factors. Data from treatment settings and community settings indicate "that moderately cohesive well organised environments that emphasise one or more areas of personal growth tend to have beneficial impact for the more well adjusted persons. For the more disturbed individuals, however, somewhat more structured and less pressured, less expression-orientated settings may be most helpful" (Moos and Finney 1984).

Their model then integrates the various groups of variables or "the boxed variables", with a new emphasis on treatment implementation; "process analysis that links program components to outcome and analyses the role of extra-treatment factors". This they argue should lead to a new way of thinking about treatment in broader terms, that integrates "formal and informal treatment resources". Citing Rossi (1983) they say these trends reflect the descriptive,

evaluative and model building activities involved in virtually all social and behavioural science research. The model has, they claim, important consequences for influencing not only "instrumental issues" in evaluation and implementation of intervention programmes but also "conceptually" in changing the way people think about the underlying disorder.

The model can facilitate "formative program evaluation" by providing data-based feedback particularly by highlighting those treatment components that are most effective as well as the perceived quality of the treatment setting (Moos 1974, 1979). Exactly the same kind of feedback can be attempted with extra-treatment factors. Current work on matching already discussed above can be improved by also using the expanded evaluation framework which gives clues for modified extra-treatment settings. The process orientated evaluation framework affords the opportunity to study drinking and relapse activity on an intra- and inter- individual basis by looking at variation over time and the influences of environmental factors such as social pressures and controls on drinking or not drinking. Certainly there is opportunity with this model to study the way environmental factors variously interact with treatment to produce or retard recovery.

In describing the work of Moos and his colleagues it will be evident that we are not only looking at sophisticated models of interaction and treatment evaluation but also moving into a much more elaborate conceptual and empirical domain. Moos is in fact one of the first researchers who has given a good deal of prominence to theoretical and conceptual issues.

However the Process Orientated Evaluation Model with its all important provision for extra-treatment determinants of outcome is limited primarily to linkages which look back to interactions with treatment. It is certainly based on assumptions about open systems but it functions at an operational level on the micro-system level. Having said this, it does provide a starting point for explanatory research which could lead to the formulation of macro-system models. A central concern of this thesis is the summative concept of Affect Balance which could be fitted to the Process Orientation Model and would provide a more macro-systemic level perspective.

There are already various sources of data at the macro-level. The survey work of Cahalan and his colleagues at Berkeley has already been discussed. Vaillant (1983) in a survey in Boston found Irish Americans seven times more likely to develop alcohol problems than those Boston residents from Mediterranean backgrounds. The anthropological literature is of course another source of macro-data. For an extensive review of this literature see Dwight B Heath (1974).

Generally social science research of the sort discussed previously has developed a social constructivist approach which is diametrically opposed to disease notions of alcoholism (Gusfield 1981). Heath (1974) characterises the constructionist approach thus: "To a significant extent, the effects of drinking are shaped by those values, attitudes and conceptions of reality, as well as by the social setting in which it takes place. Drunkenness not only has different

meanings in different cultures, but also involves significantly different kinds of behaviour. Drunken behaviour is patterned to such a degree that it appears to be, in a large part the result of a learning process". What this serves to identify is the emergence of a Social Learning Theory perspective on both treatment and aetiology as one of the main developments of the period highlighted by Heather discussed above.

I have already noted that the decade of the 1970s was characterised by the re-emergence of Social Learning Theory and its therapeutic method, Cognitive Behaviour Therapy. This was the theoretical underpinning of Heather's new paradigm in 1980. Social Learning Theory has its roots in the social behaviourism of the turn of the century, and its wide acceptance and the proliferation of research using it in the mid 1970s was something of a "rediscovery" (Woodward 1982). G. Terence Wilson (1988) says "we had little hesitation in designating 1976 as the year of cognition for theoretician and practitioner alike. Cognitive approaches to behaviour modification are riding on the crest of a wave of popularity". Psychological as opposed to the animal models which generally supported S-R psychology were introduced by Bandura (1963 and 1977). In this model, individuals no longer merely respond to various contingencies, but have self-activating expertise and learned expectations. The explanation of much more complex action is possible by virtue of these developments. The theory's overriding imprint is that of internal (unseen) cognitive processes. Self control, a key therapeutic

cognitive behavioural technique, could not be encompassed by the old behavioural psychology because the organism was seen as being under the control of environmental reinforcement. Social Learning Theory acknowledges the reciprocal interaction of a person with the environment through the mediation of cognitive processes. A key motive in Bandura's 1977 refinement of his earlier work on treatment is the expectation a person has of "self efficacy". Efficacy expectancy plays a key role in shaping and maintaining behaviour. This theoretical construct of efficacy expectation is very much at the heart of disputes over controlled drinking. If someone has an irreversible disease it makes little sense to attempt to persuade him that he should see himself as efficacious in his dealings with alcohol. We have noted already that important new Relapse Prevention strategies and programmes have been developed by Marlatt and his colleagues based on Social Learning Theory and Cognitive Behaviour Therapy. Beck (1976) laid a theoretical foundation which emphasised the organising role of negative thoughts.

Both Beck (1976) and Paykel (1987) and the respective theory and therapy associated with them are crucial to the recent developments in alcoholism treatment and indeed all other psychological approaches to mental health and life-style interventions. (See Maisto and Caddy 1981; Wilson 1980; Heather and Robertson 1985.)

Whilst still on theoretical issues, by the mid 1980s it is clear that simply labelling the Alcohol Dependence Syndrome a disguised disease notion has not in fact made it

disappear, although in the United Kingdom there is a much more tolerant reception of controlled drinking compared with the United States where research psychologists have abandoned the quest to teach controlled drinking to all but the moderately dependent drinker (Peele 1984).

The battleground between the old and the new in Britain is not controlled drinking as such, as it is in the United States but rather the Alcohol Dependence Syndrome. It is possible to over estimate the intensity of this debate and my impression is that feelings do not run as high in the United Kingdom as they do between the two camps in the United States. Nevertheless, the debate has important implications for the field of alcoholism treatment and as yet is not satisfactorily resolved. Edwards (1986) describes the origin of the core idea of the Alcohol Dependence Syndrome and reviews in great detail "studies which bear on the measurement and validity of the syndrome." He asserts: "The research which we have summarised shows that over the last few years the syndrome concept has catalysed a range of productive contributions coming from many different centres." The syndrome formulation begins to look like a useful idea. He ends with a plea for tolerance and "interdisciplinary enquiry rather than perseverations with the unproductive rhetoric of the disease debate". In the same issue of the British Journal of Addiction four invited responses to Edwards paper were also printed. Babor (1986) calls the paper a timely review which "indicates the ADS concept has not yet achieved the status of a formal theory of dependence". "To the extent that the ADS

can fulfil its promise as a formal theory of alcohol dependence, it will continue to exert a positive influence in the field for years to come". He credits the Alcohol Dependence Syndrome with having stimulated extremely valuable research.

Martha Sanchez Craig (1986), herself not a proponent of disease conceptions of alcoholism, endorses the continuing view of alcohol problems embodied in the Alcohol Dependence Syndrome: "One of the principal consequences of seeing dependence as presented in varying degrees of severity is that it raises questions concerning the appropriateness of a uniform treatment approach including uniform treatment goals". It also underlines the issue of early intervention. She argues that the syndrome has powerful face validity but presents problems "around the measurement of the proposed elements, and the use of the term 'syndrome'". She makes an interesting point about terminology which perhaps helps to bridge the disputes around the issue. It is that, if alcohol dependence is rooted in biological, psychological and socio-cultural factors, as Edwards suggests then perhaps "a term which better reflects this complexity is needed." "Patterns of alcohol dependence" might be more acceptable for multidisciplinary use.

Robertson (1986), comments ironically that "strange as it may seem, the concept of a continuum of alcohol dependence has made it politically and ideologically feasible to offer controlled drinking as a goal to some problem drinkers in the United Kingdom at least." Robertson makes

three very potent criticisms of the concept of alcohol dependence, which are instructive generally for alcoholism treatment.

1. The syndrome counts for only a small amount of outcome variance - indeed "the most striking feature of most outcome studies in alcohol problems is how little the outcome variance is explained by all of the predictive measures combined together".

2. The syndrome as false consciousness:-

Many alcoholics studied have themselves been indoctrinated with disease notions which are the prevailing beliefs about alcoholism.

3. If learning is at its heart, should the Syndrome be called a Syndrome? Robertson makes the point that despite avowals that the Alcohol Dependency Syndrome is not incompatible with behavioural analysis of drinking, it is the case that it has not fostered more complex behavioural analysis of drinking and "it has been used to foster an essentially uni-dimensional view of the determinants of alcohol problems which in substance is still a medicalised 'alcoholism'. In this respect it has been an obstruction and not a stimulus to enquiry".

The final response to Edward's paper came from Harvey A. Skinner (Skinner 1986). The crucial value of the syndrome for Skinner is the separation of the "core dependence syndrome" from other Alcohol related problems and the idea of a continuation of severity of dependence rather than an "all or none condition". For Skinner the Alcohol Dependence

Syndrome represents "the most important conceptual advance in the alcoholism field since E.M. Jellinek's 1960 classic 'The Disease Concept of Alcoholism'". This is so because it can be reliably measured and has diagnostic value i.e., abstinence versus moderation and it has inspired various experimental studies of a useful sort. The Syndrome initiates the scientific business of working towards explanatory theory - it is the early descriptive part of the process and the next phase has to be the development of theory. With the philosopher Carl Hempel's imprimatur Skinner simply asserts his approbation and goes on to suggest three research programmes that may advance theory development:

1. Longitudinal research with younger samples to identify what factors control the onset of the Alcohol Dependence Syndrome.
2. Determination of trait-state elements of the Syndrome
3. Determination of how the Syndrome and the other axis of Alcohol Related Disabilities interrelate, in Skinner's terminology how they "interdigitate".

For Skinner the Alcohol Dependence Syndrome is a stepping stone which will promote a deeper penetration and understanding of alcohol misuse. One can only comment that Skinner's overall conclusion is valid - anything that facilitates a deeper understanding is useful, but it is far from clear that the Alcohol Dependence Syndrome actually does this. Skinner does not answer Robertson's objections to be contrary.

For a detailed discussion of the seven elements of

the Syndrome see Chick (1985) and for a thoroughgoing discussion of the Syndrome, encompassing all shades of expert British opinion, see also chapter five of Heather et al (1985).

The Alcohol Dependence Syndrome is an immensely important issue for alcohol treatment and its organisation. My own view tends towards that of critics like Robertson who see the quasi-medical terminology as at least a symbolic obstacle which portends future conflict - also the paucity of explained variance is persuasive. This of course does not detract from its usefulness as an idea, but there seems to be little else to recommend it. I would also argue that the sort of theory development most needed by the field at the moment is unlikely to flow from the stimulus of the "Syndrome".

Throughout this chapter I have tried to indicate the political context of ongoing work. The Alcohol Dependence Syndrome appears to be a symbol of conflict apart from the detailed technical debates that it gives rise to. In this sense it is closely related to the notion of a new paradigm. The political strands in the debate have been recently discussed by Edwards (1985) who asks whether the paradigm shift is about the nature of the problem or is it to be "understood in terms of a competition between medicine and psychology for ownership of the field."

I wish now to discuss the only outcome review I intend to cover in this section. It is an 1986 update by Miller and Hester of their 1980 review. As such it is surely the most extensive and authoritative review available. They

tell us that in the six years since they stopped reading for their first review more than 300 new treatment reports had been published. They make a link with the first review at the outset when they tell us that "a number of important controlled investigations have appeared, and the information available on how to select optimal interventions is still stronger than it was in 1979. Yet, as far as we can see, this research has still had virtually no effect on treatment practices in the United States, where alcoholism treatment has become a major profit making industry". Their chapter is restricted to a consideration of controlled evaluations where drinking behaviour is the focus of attention. The review is organised around different treatment methods with a final discussion of treatment length, setting and matching. I will follow the sequence of their chapter, but will ignore those aspects covered adequately in the first paper.

Drugs : "Although there has been a staggering number of studies of drug therapies for alcoholics, there have been surprisingly few controlled investigations that have included an adequate outcome measure of drinking behaviour". Drop out rates are around 50%, and they present difficulty for interpretation of findings. "Few studies have demonstrated the effectiveness of antidipsotropic agents". "Their impact can usually be attributed to placebo effects." Finding about antidepressants including lithium: "They may decrease the desire for alcohol in some alcoholics but at present it is not possible to match sub-population with the agent concerned".

Psychotherapy and Counselling: Various studies of in-

patient and out-patient settings are reviewed. Curiously, hardly any of the work which was reviewed is of recent date. Where differing approaches were contrasted the absence of untreated controls makes it impossible to assess if they were superior to no treatment at all. "Viewing the controlled and comparative studies as a whole, we are struck by the absence of consistent and substantive support for the efficacy of traditional psychotherapy and counselling approaches as evaluated to date". Although groups and individual counselling are among the most popular methods used in the United States as well as Britain, "there is little or no evidence to date that such interventions have a specific long range impact on drinking behaviour".

Confrontation: There is a widespread belief that confrontation is a necessary component of counselling the alcoholic. However Miller and Hester could not find a single controlled evaluation of confrontational counselling with alcoholics, despite "many dozens of descriptions" of how to do it.

Alcoholics Anonymous: "In spite of the fact that it inspires nearly universal acclaim and enthusiasm Alcoholics Anonymous wholly lacks experimental support for its efficacy". Not a single controlled evaluation has been reported to date.

Alcohol Education: A component of most recovery programmes, controlled studies with random assignment have not supported the idea that Alcohol Education has any value for changing drinking behaviour and problems.

Marital and Family Therapy: The reviewers note that

"therapists and programs have increasingly included the spouse and other family members in the treatment process" and "a few controlled evaluations have been published to date, with mostly encouraging results". It is interesting to note that of the seven controlled studies reviewed three were published after 1980. Their conclusion about these studies are also interesting; it is that family therapy as an adjunct to other treatment improves outcome at 6 months follow up. Its failure to sustain improved outcome over longer periods "appears to be due in part to the gradual improvement of the comparison groups and in part to the erosion of gains following marital therapy". Miller and Hester consider that this modality has consistently produced positive findings and warrants inclusion in treatment programmes.

Aversion Therapies: With aversion therapy alcohol is paired with an unpleasant experience, so that the individual develops an automatic aversion or negative response to alcohol. A variety of unpleasant events have been used - four sorts are reviewed by Miller and Hester

- 1) Nausea
- 2) Apnea
- 3) Electric shock and
- 4) Imagery (including hypnosis).

Nausea was the first technique used, usually being chemically induced. Although there have been many uncontrolled reports of an optimistic sort, there has been only one controlled study to date of chemical aversion published in 1978. Lithium was the agent used. It did no

better than controls at follow up. We are not told what the controls were.

Apnea was a horrific technique used during the 1960s. Succinylcholine, a drug which causes a total paralysis of movement and breathing for approximately 60 seconds, was given by injection. During the phase of paralysis the individual is given alcohol by having it put on his lips. Apneic Aversion has been shown to be superior to a placebo, but the terrifying nature of the technique ensured that it was not used much outside experimental situations in the 1960s.

Electric Aversion: The advantages of electrical technique have been mentioned in our discussion of the 1980 review. The present one merely underscored the previous conclusion that electrical aversion has been more successful in reducing consumption than in achieving abstinence. The reasons for this specific action are not known at present. Miller and Hester suggest that since this method is painful and produces high drop out rates alternative methods of proven validity should be used.

Hypnosis: Two studies found in favour of hypnosis and two other more methodologically reliable studies found against it. It was therefore not possible for Miller and Hester to pass judgement.

"Research on the aversion therapies constitutes one of the largest literatures in the alcoholism treatment field". They conclude that aversive conditioning works "at least for a period of a few months". "Reduction of consumption rather than total abstinence is a common observation following

aversion therapies." It is recommended for inclusion in treatment programmes.

Controlled Drinking: This is of course a treatment goal rather than a treatment method, however, Miller and Hester review controlled outcome studies of programmes for controlled drinking. The first published study was that by Lovibond and Caddy in Australia in 1970 discussed previously. This study was flawed by high attrition in the control group. A further study by these researchers in 1976 produced an 80% success rate at 12 months compared with 60% for those patients not receiving the electrical aversion component and 30% for those who did not receive the self-control training. They comment that comparative evaluations yield "strikingly similar findings".

Behavioural self control programmes for controlled drinking tend to produce "two thirds being rated as successful outcomes" at one year follow up. There have been a large number of behavioural self-control studies of controlled drinking which have met acceptable methodological standards. With out-patients "improved rates of 60% to 70% have been found at follow up as long as two years." (Miller and Baca 1983). Miller and Hester qualify their support for controlled drinking by suggesting that it should be directed at those individuals who have not established high degrees of dependence (Miller 1983). Chronic alcoholics by contrast present "a rather mixed picture"; negative findings with controlled drinking have come from this population. Current data (e.g. Fog et al 1984) indicate that controlled drinking

training is not an effective method for chronic alcoholics who are severely dependent.

Operant Methods comprise a large literature, but most are laboratory studies. In vivo studies have mostly been restricted to case studies. However, what literature does exist indicates that drinking behaviour is responsive to the contingencies of reinforcement discussed earlier, including the use of punishment to coerce individuals into treatment, usually drunk drivers and referrals from industrial programmes. The coercion is effective in getting people into treatment, but outcome depends upon the effectiveness of the programme into which people are directed.

Broad Spectrum programmes focus on life problems in addition to Problem Drinking, because drinking appears to be related to these life problems. These broad spectrum programmes which tend to focus on skill defects emerged in the early 1970s. Several studies have evaluated the impact of adding social skills training to treatment programmes and uniformly successful results are reported. These methods, which include stress management and social skills training as well as marital communications training come into play once an individual has stopped or reduced his drinking. The goal is then to maintain the gains. The most important broad-spectrum approach is the "Community Reinforcement Approach". "If one were to judge the effectiveness of alcoholism treatment methods based on the strength of scientific support available for them, the Community Reinforcement Approach (CRA) would surely be top of the list". It aims to control family, social

and employment reinforcers so that sobriety is reinforced. Miller and Hester express what appears to be surprise at the fact that despite the obvious value of CRA "it remains little known and seldom used". The problem is that it is a very expensive enterprise in which to engage and this explains its infrequent use. They conclude that Broad Spectrum programmes, in particular social skills training, stress management and community reinforcement approaches are useful for those who need them.

Miller and Hester illustrate their conclusions in a table, viz:

<u>Research Supported Treatment</u>	<u>Unsupported Treatment</u>
1. Aversion therapies	Alcoholics Anonymous
2. Behavioural Self Control	Alcoholism Education Training
3. Community Reinforcement	Confrontation Approach
4. Marital and Family Therapy	Disulfiram
5. Social Skills Training	Group Therapy
6. Stress Management	Individual Counselling

Three basic principles are offered to help therapists design future programmes:

1. Choose only those components supported by controlled outcome research, i.e. those listed above;
2. Offer the least "intensive and intrusive" first, only using more expansive and elaborate methods if these fail;

3. Individuals should be matched to optimal treatment on the basis of "predictors of differential outcome".

"Clients should be informed participants in their own treatment planning process, and should be offered a range of plausible alternatives along with fair and accurate information on which to base a choice."

With respect to Miller and Hester's review it is worth reiterating the point that no study published to date has accounted for more than a little of the outcome variance. Bromet and Moos (1977) put the figure at between 10 - 33% and outcome measured by abstinence alone tends to be predicted less efficiently than when more complex clinical outcome scores are used (Schuckit et al 1986). A recent British study (Elal - Lawrence et al 1986) which confronted the confusion in the literature on predictions of outcome suggested three key variables;

1. The subject's cognitions
2. Past behavioural expectations and experience of abstinence
3. Freedom of "choice of treatment goals".

Conclusion

In this chapter I have reviewed the treatment outcome literature from its beginnings to the present time. In doing this I have endeavoured to let authors speak for themselves by the extensive use of quotation. This partly relates to a further objective of this chapter which is to illustrate the various developments and debates in the field that impact upon treatment. It is always useful to contextuate research

studies: however it is a contention of this thesis that the particular development of professional involvement in the field of alcoholism studies is a key variable in understanding the current models and treatment practices extant and hence the treatment evaluations undertaken. This perspective may also give rise to insights for future research directions.

It has been stated that alcoholism treatment gets something of a bad press right up to 1980 when affirmative comments begin to appear in the literature. I commented upon how the explosion of research in the 1970s was closely tied to the increasing presence of psychology in the field, which itself has created something of a contest between psychiatry and clinical psychology. Noted also were differences between the state of affairs in Britain and that in the United States where the conflicts appears to be between the "alcoholism movement" and others who espouse the "new Paradigm" based on social learning theory. The inter-disciplinary rivalry in Britain, at least for some however, subsumes this antagonism of old and new models of alcoholism. Throughout, the paucity of theory has been noted, and in particular theoretical constructions of basic social science to the detriment of productive explanations of the problem of alcoholism and its treatment. In this regard the pre-eminence of empirical, data-driven research has been noted. This has not been unrelated to the ideology of the professions involved.

Particular attention was focused on non experimental community based research and the Process Interaction Model which enables extra-treatment variables to be included in the

evaluation of outcome. It was noted that social context factors employed in models like the Process Interaction Model showed particular promise for outcome research. However, it is my opinion that as yet, an appropriate balance between person and social context variables has not yet been achieved by researchers. Nor has an adequate means of collecting data, that is focused upon person-situation interactions. I hope to make a small contribution to resolving this shortcoming, by presenting both the measure of the Life Domain Affect Balance Scale, as a suitable research instrument to sample social context variables and the concept of Affect Balance as a theoretical tool which could assist researchers to better explain person-situation interactions in treatment outcome research.

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CHAPTER 2

AFFECT BALANCE

In this chapter I wish to introduce the concept of Affect Balance: its measurement and utility as a research tool with theoretical, clinical and empirical promise. Its theoretical properties and empirical supports will be explained in the context of relevant philosophical and psychological work on evaluation in a separate chapter where its utility as a research tool for related fields of social science research will be discussed.

Before proceeding to describe the concept and its measurement I will create a context by making a few comments about the literature reviewed so far, and about relevant developments in the social science literature which bear upon my purpose. In Chapter One a vast literature on alcoholism treatment was sampled. This literature was itself embedded in more wide-ranging debates about the nature of alcoholism. In reviewing evidence concerning treatment efficacy, the first step is to consider the quality of such evidence, and then its relevance. These concerns are considered under the well-established canons of validity and reliability. I followed the progress of outcome research over an extensive period, indeed, the period starting from its origins as a scientific, or should one say quasi-scientific concept to the present time. Throughout the review, objectivity was not felt to be extant as a consequence of study methods. An improvement in such methods over the last decade was noted.

However, it would be naive to imagine that all problems

would be clarified and resolved merely by the adoption of more rigorous scientific methods. Validity and reliability are not simply issues of scientific rigour. These concepts are themselves enmeshed in wider philosophical debates concerning the nature of the scientific enterprise. Some of the influences that shaped the research reported were identified. It was seen that the theory-grounded observations of researchers and their definition of what counts as evidence, is itself an open-ended debate.

At the present time it is true to say that there is no strong consensus about the nature of the beast that they studied, nor how best it might be studied. As such, it is my view that essays at novel conceptual frameworks are required, and that a shift in focus in the way the content of the alcohol literature is read might be fruitful in the detection of new insights from this and related fields of endeavour.

Without reiterating the methodological criticisms of Chapter One, a few comments are nevertheless warranted to provide a context for the main theme of this chapter. Theoretical developments over the last decade within the alcoholism field and indeed further academically abroad, have resulted in models of behaviour and aetiology that take account of both public and private events. Note has already been made of theoretical expansion involving the inclusion of social environments from learning theory circa 1950 (Conger 1956) to Social Learning theory of the 1970s (Bandura 1969, 1977), to cognitive Social Learning theory from 1970 to 1980 (Meichenbaum 1977, Bandura 1978, Marlatt & Gordon 1985). The

acknowledgement of social context models for all sorts of behaviour including alcoholism is widespread in social science literature (Dembo 1981). Of course for social science the significance of the social and cultural environment is taken for granted. However, the divide between sociological and psychological evidence has been and continues to be problematic. Notwithstanding academic rivalries, this is an issue of both methodological and theoretical import. Social Psychology is a discipline which traditionally has straddled the border between private and public events, but it has had little or no success in really getting to grips with this issue. We have seen the emergence of Social Learning theory from mainstream psychology; it enlarged the psychological perspective on learning such that all the traditional categories of learning, from classical conditioning to higher level processes of learning, were subsumed, so that it is possible to conceive of an individual's appraising and responding to complex social and environmental wants. In short, Social Learning theory takes cognisance of social contexts from both a public and a private perspective.

The importance of the environment for mental health has been demonstrated by early work, itself influenced by classical sociological theory. Robert Park wrote about social marginality in 1928 and focused attention on social isolation (Park 1928). In this work Park drew upon an earlier model produced by Simmel (1950). Influential work by Faris (1934, 1938) and by Faris & Dunham (1939) provided both theoretical and empirical demonstrations of the link between social

isolation and mental illness. Schizophrenia particularly was found to be prevalent in the more socially disorganized zones of the city centre.

Although this work dealt with clinical issues, it nevertheless emerged from Sociological theory. More clinically oriented surveys were undertaken: in the 1950s Hinkle & Wolf (1957) demonstrated the relationship of recent life-events and illness. This and other research undertaken in the United States made it clear that mental health professionals in hospitals were not the agents of choice favoured by most Americans to deal with their mental health problems. These services were provided by family and friends, neighbours, clergymen etc (Gurin, Veroff & Field 1960). These informal carers and community "gatekeepers" were recognized as a potent force in mental health and influenced new initiatives in what came to be called the Community Mental Health Movement in the 1960s (Caplan 1963). This movement gave an impetus to research which focused on person-environment interactions on a clinical plane as opposed to the hitherto more abstract and theoretical positions of academic psychology and sociology. A good deal of clinically focused work was to follow in the 1970s' on environmental stress, life events, coping and social support.

The need for new initiatives had been underscored by an early survey which highlighted the extent of mental health problems. In the Stirling County study, only 17% of the study population was symptom free (Leighton et.al. 1963). In the 'midtown Manhattan study', nearly one quarter of the sample

displayed serious symptoms (Srole 1962). These early and extremely influential mental health surveys also demonstrated that sociological variables involving social class, culture, age and other demographic variables were important in explaining rates of mental illness and health. In the United Kingdom the Harlow New Town Study (Taylor & Chave 1968) showed how various individuals from the central London area were affected detrimentally when they were transplanted into the quite different culture of a New Town. By the beginning of the 1970s, the study of social support had become a discrete research field in its own right (Cassel 1974, Caplan 1974, Cobb 1976). Social networks have been studied together with the family as a source of health which protects and promotes psycho-social resources (Henderson 1977). During the 1970s great enthusiasm and high expectations flowed from the concept of social support. In the 1980s it has lost some of its gloss but it still remains a crucial research domain (for a review see Wortman 1984).

At present, the study of life-events in relation to health and illness has the dimensions of a major industry, although it is true to say that the promise of this line of research has been only partially realised. Whilst general conclusions about the importance of environmental events for the individuals who experience them is not in question, quite what sorts of events experienced by what sorts of individuals, and amid what sorts of contextual factors, is not clear. It is akin to looking through a fractured lens. In short the consensus is about the importance of an individual "life

course"; such a conclusion is hardly startling. With regard to life events and alcoholism, not much work has been produced. For an up-to-date review see O'Doherty & Davies (1987). Among the conclusions drawn by these reviewers, is an acknowledgement of a failure to elaborate upon the dynamic aspects of the dependence-environment interaction, and the absence of an integrative, testable theory. Also, it is my view that the crucial point of life events is that they require to be treated as but one part of a larger theoretical edifice. It may well be claimed that these three issues have more than a little overlap and that they reflect the main theoretical question already raised, namely that of handling both public and private events.

Having made these cursory comments concerning the centrality of social contexts as perceived by individuals, and the apparent shortcomings of current methodologies for dealing with the person-situation interactions, I will now introduce the Concept of Affect Balance which, I shall argue, is a useful concept that has potential for enhancing our ability to analyse person-situation interactions albeit through modest, post hoc explication. What this problem of person-situation interaction points to is the absence of an integrative theory. Put another way, progress would, I feel, be enhanced through the adoption of some overarching concept that might facilitate more effective empirical work and represent a major transition-point in the direction of theory development.

Overarching concepts seem to me to be necessary in a dynamic social universe. Wherever progress is made, and in

whatever subject-domain, there is an increase and diversification of knowledge. For instance, new knowledge consequent upon developments in scientific methods usually tends to produce an imperative for the subsuming and simplification of the results of science in the service of explanation. At the beginning of this century, Bertrand Russell, in his essay, "Mysticism and Logic" noted, "Every advance in a science takes us further away from the crude uniformities which are first observed as a greater differentiation of antecedent and consequent, and thence into a continually widening circle of antecedents recognized as relevant" (Russell, 1918/1963). Similarly, E.H. Carr, in discussing the task of the historian, suggests that the historian like the scientist needs to simplify the multiplicity of facts encountered; to introduce unity into the chaos. "History like Science advances through this dual and apparently contradictory process" (E.H. Carr 1961). It is evident that social science problems described mirror those of natural science in this regard.

Affect Balance as a theoretical construct has not, to my knowledge, been discussed in the social science literature. It emerged as a theoretical concept in the work of Bradburn and Caplovitz (1965). This work was not part of the Community Mental Health Movement research tradition mentioned above, but rather belonged to the related and parallel "Social Indicators Movement" of the 1960s. This movement produced a series of pioneering studies of the Subjective Quality of life using "Social Indicators" as opposed to Economic Indicators.

In this regard, happiness, life-satisfaction and psychological well-being, are cognate terms in the context of this early social survey research. It is interesting to note that, prior to the National Opinion Research Centre studies of Happiness, no empirical investigation had been undertaken on the link between social integration and feelings of well-being or happiness.

Bradburn first hit upon the idea of Affect Balance accidentally when examining data from his happiness survey. He found that an individual's score on a measure of positive affect did not correlate negatively with that person's score on a measure of negative affect. This ran counter to what had been presumed on a common sense basis. Namely, that happiness could be described on a continuum with one pole being very unhappy and the other pole very happy. That is, positive and negative affect are negatively associated. In his original survey, Bradburn had indices for both constructs, and instead of the expected negative relationship, he found in fact, no relationship at all. Indices of positive and negative affect were statistically independent so, instead of a continuum model of happiness, what was observed was a two-dimensional model. This in fact, is the essence of Affect Balance. Ratings of happiness can only be achieved by obtaining a measure of positive and negative affect, and then subtracting one from the other to obtain a balance score.

Initially, Bradburn and his colleagues had been attempting to measure the mental health of survey respondents by developing measures of stress arising from daily life. The

negative affect-scale was an index of "difficulties in living" in daily situations as opposed to a measure of longer-term problems. Self-reports of life satisfaction were indices of mental health. The discovery of the balance score as described above gave the notion of "measure" a much wider significance than its original function in a mental health survey.

An implicit measure of mental health follows almost automatically from the measure spoken of, the Affect Balance scale (Bradburn 1969). That is, that psychological well-being is contingent upon the actual achievement of positive states of well-being and not merely the absence of negative states. According to this view there is no natural state of health which comes without having been positively achieved; no natural state of happiness. This possibly has theological implications, but what is important for the present discussion is to understand the nature of the mechanism. Bradburn's actual scale was quite simple: five items to measure positive feelings, and five items to measure negative feelings. The scores are summed and subtractions made, in order to provide the Affect Balance Score.

The dimensionality of the measure then, is the crucial point to grasp. Theoretically, therefore, if a person were to suffer a series of catastrophes such that on a common sense view they would be thought to be justifiably miserable, but had enough positive events, which to their subjective appraisals were capable of compensating for their negative experiences, they could achieve a positive balance score and

be said avowedly to be happy to an extent. The statistical independence of the two scales is what is important here. In its original use, in keeping with the survey's aims of tapping feelings of a day to day sort, the time period over which the respondent was asked to report was short: "the past two weeks".

An important support for the measure of Affect Balance, was the fact that the two separate scales of positive and negative affect, were differentially correlated with variables concerned with, *inter alia*, social participation. Negative affect correlates with indices of poor mental health, like anxiety and illness, etc., whilst positive affect is correlated with social participation and feelings of well-being. It is of course not surprising that this pattern of correlations should be observed since they are implicit in the very language in which the questions are asked.

What is of paramount importance is the independence of the two measures of affect when, as I have noted above, this appears to run counter to commonsense assumptions. Bradburn's model aroused and continues to arouse interest, particularly among researchers involved in producing subjective social indications in the United States and in the United Kingdom (Andrews & Withey 1976).

Harding (1982) replicated Bradburn's scale on a British urban sample and tested it for relationship to measures of satisfaction and psychological adjustment. He also tested the psychometric properties of the scale using powerful statistical techniques not available to Bradburn at

the time when he first analysed his data (Principal component and factor analysis followed by various rotations). The Balance model did indeed emerge from Harding's survey where Bradburn's scale items had been included with other survey questions. "Two distinct factors emerged with the five positive items loading on one factor, and the five negative items loading on another". The results very closely resembled those obtained by Bradburn with an American sample.

The pattern of differential correlation, with various other relevant variables, was also borne out, although in the British data, social participation (which was only measured by one item) was related to a decrease in negative affect as well as by an increase in positive affect, whereas in Bradburn's balance model, it had been exclusively related to an increase in positive affect. "Bradburn's Two-dimensional model of affect was successfully replicated in the British study". Indeed, additional support was provided for the model from additional items in the British study.

Harding then argues that merely because the scale is psychometrically sound, we are not obliged to accept Bradburn's argument that well-being is environmentally determined. He argues that demographic and personality factors can be obscured in averaged balance-scores. The individual items of the scale may well be mediated by personality traits as for instance would be predicted from my earlier glance at Social Learning Theory. The important point is made that the use of a short time period of a few weeks as a temporal referent does not partial out a trait response from

a state response. Further work by Costa & McCrae (1980), is cited by Harding to illustrate the fact that personality is implicated in a person's responses to the scale although the evidence available tends to show that personality variables do not account for a large amount of response variance. Transient situational factors are by no means invalidated as predictors of scale scores by personality factors. Harding suggests a more complex model which takes account of personality as well as routine events called for in explaining psychological well-being, instead of Bradburn's singular model.

The inclusion of psychological variables as an implicit component of an independent response is an essential aspect of what I want to suggest is most valuable in viewing the concept of Affect Balance as an overarching concept straddling both public and private events, as mentioned briefly above. With regard to the empirical work described in the next chapter on methods, it is also non-problematic since it is the theoretical construct which is called upon in this thesis and not the actual scale. My modification of Bradburn's scale is described in the next chapter on methods.

Overall, there is substantial support for Bradburn's Balance Model of Psychological Well-being in the British survey. At this point, in introducing the Affect Balance Scale as originally designed by Bradburn, it is appropriate to take account of an important paper by Cherlin and Reeder (1975), not least because Bradburn himself has concurred with some of their criticisms. These authors criticise Bradburn's

model because it is predicated on the fact that the balance scale has a higher correlation with self-reports of happiness and life satisfaction than either of the sub-scales taken singly. Since the co-efficient of correlation does not differ greatly between the balance score and each of the sub-scale scores, (0.42 as opposed to 0.32), the evidence is unsatisfactory. This small disparity may be due "simply to summing the effects" of the sub-scales rather than an underlying balance process. They also argue that the Ad Hoc nature of the scale's construction is at variance with traditions of psychometric research which further invalidates the scale, although they do not say precisely why this is so. They raise serious criticisms about the validity of the concepts of negative and positive affect and say that this makes the theory deficient for a theory of well-being. Bradburn's work is also partially replicated with data from independent surveys.

Cherlin and Reeder make what I consider to be a telling criticism of the Affect Balance Model when they examine it from a perspective informed by psychological writings on emotion. They make the point that Bradburn's research was originally focused on the measurement of happiness and its correlates. However, they assert that happiness in the psychological literature is usually defined in terms of "hedonic tone" and/or elation- depression, and indeed that both of these definitions might be but one aspect of emotional flux. Many and varying feelings might be implicated in "feeling happy". They cite examples from

research for dealing with happiness and suggest that the twin notions of pleasant-unpleasant, and level of activation, are ubiquitous. Bradburn appears to stand accused here of being out of the mainstream research tradition. However, it is not suprising that varying types of psychological research (tradition-bound) tend to agree with one another insofar as saying that two fundamental dimensions of happiness might be labelled pleasantness and activation.

It is noted that Bradburn and Caplovitz broadened the range and the name of their dependent variable from happiness to psychological well-being - where the umbrella term "well-being" includes many feelings which are not just pleasantness - happiness. Having widened the conceptual frame of feelings which were assumed to be tapped by the scale, Bradburn did not widen his model of feeling states accordingly. Bradburn is accused of ignoring current work on the multiple dimensions of "affect, mood or personality" and was seen as stubborn because he sticks to his hedonistic conception of positive and negative affect. The items of the scale itself are said to suggest that the positive scale measures activation as well as pleasantness, while the negative scale measures pleasantness-unpleasantness. In summary, they suggest that the model is too simplistic to handle the complexity of emotional feelings. With regard to the crucial issue of independence of positive from negative affect, they suggest that the twin assumptions of independent feeling states which can vary independently of each other, may be incorrect and that the observed independence might be explained as a function of the items

chosen rather than as a reflection of an underlying balance process. The two feeling states of positive and negative affect may well be associated with each other as a commonsense continuum model suggests, but this association is obscured by the particular focus of the items of the scale since "at least half of the ten items can be classified as referring to fairly specific situations". The independence of the two "hypothetical subjective feeling-states" empirically derived from Bradburn's original work, and much subsequent replication, may on this reasoning be a measurement artefact.

There is much scathing criticism here of Bradburn's model and measurements, but I am not sure that it is all as devastating as it appears. Cherlin and Reeder having attacked the underlying rationale of the model, then go on to complete the task by questioning the utility of the global Affect Balance measure. Indeed in this they have the agreement of Bradburn himself who wrote to the authors agreeing that the combined scores did not add much and supports their conclusion that the combined Affect Balance score as opposed to the two sub-scales is of "questionable marginal utility". Bradburn does not however, accept their conclusions about the possible invalidity of the underlying balance model.

Having said this, Cherlin and Reeder present a valid critique of the Affect Balance measure and its underlying model, but one which does not demand acceptance as reliable apart from being interesting and suggestive. Most of their criticisms are in fact unsubstantiated by data; nor indeed in the nature of things as they are at the present, could it be

otherwise.

Indeed, a discussion of Cherlin and Reeder's paper provides illustrations of precisely why I think that my modification of the scale might be valuable as a methodological tool: it allows one to subsume sub-sets of complex data and helps make sense of a multiplicity of scientific data, as advised by Russell and Carr mentioned above. To address more specific issues raised: the Cherlin and Reeder replication study did in fact satisfactorily vindicate Bradburn's results where these had been tested. Indeed, it also replicated the Harding study by implicating personality variables. In this latter case the relevant variable was external-internal locus of control. The involvement of personality with individual evaluations of a state of well-being is not a problematic issue for Bradburn who has conceded that his model could profitably be expanded to include personality variables. For my purposes, such involvement is essential, since the manner in which I conceive the concept of Affect Balance is that of forming an "arch" between the person and his/her environment. One pole of this construct is located in the person, whilst the other is embedded in the social environment. It might also be added that this state of affairs is posited in almost all theories of the emotions.

In respect of the somewhat overextended use of the term "happiness" and its further translation into "psychological well being", the comments by Cherlin and Reeder are apposite as far as they go, but their conclusions appear

to me to invoke a category mistake. No doubt their reading of the psychological literature is correct, and it is the case that Bradburn takes some semantic licence by cramming a complex of psychological feeling-states into a referent describable by a single term; he then extends this process by using a single generic term to hold a complex construct: happiness. The fact remains that the hedonistic frame of reference which underpins Bradburn's model was used in a post hoc fashion after the data were in, as it were, in much the same way that clusters are labelled rationally in factor analysis. Not only is it unfair to attack an empirically derived construct as if it had been generated theoretically - it is a fact that empirically, it works. Subsequent work found the same pattern of results.

The most telling criticism is that the items, because of a degree of serial dependence and situational specificity introduce an element of tautology into the measure. Given the extent of external validation when the concept does the work of an higher order construct, as opposed to a reductionist one, it does appear thus to have been weakened somewhat. Paradoxically, from the point of view of theory construction, it is its use as an overarching concept that is most valuable for an operational definition, in a middle range theory as opposed to a piece of reductionist research on emotion. The Bradburn items can be assumed to subsume the complex activity involved in feeling-states and emotion which, in any event are still but poorly understood in terms of mainstream psychology. Attempts will be made in Chapter Seven to root Affect Balance

in a range of relevant core concepts of emotion and relate it to theoretical and empirical works in psychology. It is hoped this will lend support to the concept and measure of Affect Balance as used in this thesis, such that some light might be shed upon the complexity involved in measuring complex feeling states.

With regard to Cherlin and Reeder's criticism of the "difference or discrepancy score", that the correlation of composite indices with a third variable creates difficulty in interpretation insofar as knowing what is correlating with what, this really does no more than to state a truism for this sort of analysis. It is clear from Cherlin and Reeder's discussion of the empirical and theoretical difficulties in the interpretation of scores on the Affect Balance Scales, that the measure itself is not without flaws, but none of the Cherlin and Reeder criticisms invalidate the instrument, much less the underlying balance model. When the instrument is used in a middle range theory, some of the very faults outlined in the Cherlin and Reeder paper become virtues. The sub-scales, or at least those in the modified version that I wish to present in the next chapter on methods, subsume clearly complex emotional and cognitive activity which, at the moment, is still only poorly understood. The fact that state and trait phenomena are sampled simultaneously in no way weakens the concept. Indeed, since it captures something of the flow and flux of emotional judgements, it is in this way enhanced. There is an implied criticism of Bradburn for not invoking a valid theory of emotion, but the state of play in

that respect at present, is that there is certainly little consensus about what would constitute a comprehensive psychological theory of emotion, although the social constructionist theory proposed by some sociologists tends, if anything, to support Bradburn's model.

I have attempted to outline the need to develop methodological tools which might help us make sense of person - situation interactions, and to extricate ourselves from the morass of empirical data which collects apace behind disciplinary boundaries, and which tends to be somewhat impermeable. I have suggested that the work of Bradburn might provide a useful concept to create one sort of bridge. The important critique of Cherlin and Reeder is instructive in its own right insofar as it points to one of the difficulties with the overall problem of being discipline-bound. They approach the problem quite rightly from one perspective and review the difficulties in using Affect Balance as a scale to measure psychological well-being in a reductionist research enterprise with the object of gaining a fine-grained understanding of how individuals come to make judgements of their own feeling-states. The more analysis that they do, the more complex the procedure presents itself as being, hence they quite properly, from their point of view, demand a more elaborate model of the entire process. The difficulty with this is that the whole can become overly complex unless judgements are made on the basis of an adequate theoretical understanding of the precise processes involved. It is the case that in the English language there are approximately 4,500 psychological trait-

terms in existence (Eysenck 1978), and over 500 terms used to describe emotional states (Averil 1975). Clearly, like the physicist who delves deep into the intricacies of the atom and ends up with anti-matter and "charm", the psychologist could follow a similar path, but to much less effect in terms of validity and substantive meaning.

A relevant theory of emotion for the present discussion has been propounded by the sociologist T.D. Kemper (1987). Kemper takes account of the fact that autonomic and cognitive activity is involved in emotional states. His theory posits that a limited range of autonomic activity underlies an almost infinite variety of culturally constructed emotive feeling states: "As long as Society differentiates new social situations, labels them, and socializes individuals to experience them, new emotions will continue to emerge". This great variability is primarily of a cognitive nature because the limited range of autonomic activity co-extensively sets limits to the actual range of experience of the emotions. He argues for four basic, or primary emotions:

A. Fear. B. Anger. C. Depression, and D. Satisfaction.

Secondary emotions such as guilt, shame, pride, love, ennui etc. are said to be acquired through socializing agents while the individual is actually experiencing one of the four primary emotions at the autonomic level. He further argues in support of this theory that the primary emotions, in contrast to the secondary ones, are :-

A. Evolutionarily important B. Cross-culturally universal
C. Ontogenetically early to emerge in the human infant.

The four primary emotions he says are linked in an empirical way to important outcomes of social relations. It is evident that this theory of emotion links very elegantly with Bradburn's model of psychological well-being, and at the same time indicates the infinite regress that the reductionist methodology of Cherlin and Reeder might encounter once attempts are made to differentiate the possible 4,500 traits as independent variable or the 500 emotion-terms as dependent variable. Again, a theory such as this buttresses the importance of having middle range theories with methodological constructs such as Affect Balance. Consideration of the relationship of Affect Balance to more fundamental theoretical aspects of emotion will be postponed until Chapter Seven, after the data analysis, where it can be linked with the results of this research and other related fields of research in a more rounded and instructive manner.

Before leaving this detailed account of Bradburn's work and critiques thereof, since in this chapter I have tended to defend it on conceptual grounds as offering promise to researchers involved in extensive investigations, I want to show that it is also the case that alternative explanations are available to account for the independence of positive and negative affect scales: a crucial component of Bradburn's model. Three disparate but non-competing explanations have been produced by Ware et. al. (1974), to account for the hitherto unexplained finding lying at the heart of Bradburn's model. All three explanations receive empirical support from his survey of British university

students. The first explanation explored the possibility that positive and negative episodes in the lives of a student population were independent and could be viewed as parallel examples of a single generalization. Life event surveys measured positive and negative events in the social environment and a modified Affect Balance scale measured feelings experienced. Their results show that the original Bradburn results may be subsumed under an higher order generalization about psychological well-being or happiness.

The second explanation is much the same as Cherlin and Reeder obtained and argues that once the response-mode is changed from a fixed sum framework to one where the affect items are held constant, and the response and scoring procedure varied to allow for frequency of occurrence, the inter-scale independence is removed.

The third explanation looks at the results of measurement of short term affects as being dependent upon fixed personality dispositions. "One set of dispositions is responsible for positive affect.....whereas another independent set of dispositions influence negative affect." This is an hypothesis originally formulated by Costa and McCrae (1980) and has been noted earlier.

The Ware paper is of course one thing, and the mass of replication data of Affect Balance quite another. What appears to be fairly certain is that a positive Affect Balance score is indicative of good mental health, and that the properties associated with the positive affect scale, like social participation, are some of the very properties

responsible for producing and maintaining good mental health. It should be remembered that Affect Balance is a theoretical construct which can be operationalised in more than one way to produce a subjective social indicator. Its component parts can be both aggregated or disaggregated to suit the particular research purpose. The Affect Balance Model has been shown to work. As a model it does not necessarily have to mirror reality exactly; it is not a theory, although we have seen that it has sometimes been evaluated as if it were. Its purpose is to simplify reality without losing anything of import. The Affect Balance model is like a shell, its contents are provided by theory-derived data. It acts by relating the data to the theory through a process of theory construction.

To recapitulate, we have a theory - Social Learning Theory - which is a systematic approach to the way that human beings act, think and feel. This includes within its compass categories of learning, a view of the way people are influenced from primitive conditioning through a variety of levels of increasing complexity to the way people learn by social interaction. But, at the higher levels, learning is not simply conditioned, but mediated through complex cognitive activity.

Next, I gave a brief description of sociological work, particularly early research demonstrating how certain classes of sociological data influenced individuals; how a person's social life was crucially important for his psychological and social well-being. Differences were noted in the conceptual

apparatus used in the domains of psychology and sociology such that the two did not easily match together in an explanatory framework or theory. I noted that in both domains progress in terms of attempts at a rounded explanation of behaviour tended to be impeded by the atheoretical accumulations of data. This was particularly so where unrestrained reductionist methodology was involved. This state of affairs, I argued, demanded that attempts be made to construct theory which would further progress by integrating the two domains: the public and the private. Such a theory, tried-and-tested, is of course no-where to be found - it is an ideal towards which to work. I then described the concept and the model of Affect Balance which I described as a way of accounting for both public and private events. As a subjective social indicator it has the properties of being able to account for any amount of social/public activity which is capable of being appraised in subjective terms. As a construct then it can act as an overarching concept that incorporates important psychological phenomena like personality as well as sociological categories like work, leisure, family etc. As a model, it can have data fitted to it and can thus enable one to test hypotheses about person-situation interactions, and aid the development of a much needed integrated theory. For the purposes of this thesis, the theory concerned is one concerning recovery from alcoholism.

I have not written much about alcoholism in this chapter, because it can be conceived of as only one class of problem behaviour, and as such is no different from any other

within the context of the discussion overall. It should be apparent that a close link exists between the recovery model of Tuckfeld that was described in the first chapter and the employment of an Affect Balance measure to be used in the data collection.

I have postponed the description of the actual instrument used until the next chapter since it differs considerably in form from that of Bradburn, but adheres nevertheless to all the essential properties of his balance model. As I suggested in Chapter One, the alcoholism treatment literature has, in my opinion, become very insular and in some respects has lost its way. When this happens, it seems eminently sensible to return to a source model and start again. Along the way, work on alcoholism treatment evaluation has provided some insights into what the components of the source model should consist of. Again, important research efforts like those of Tuckfeld have been helpful in providing a view of what is involved in the recovery process. It will of course be no surprise to learn that the important elements in alcoholism recovery look very much akin to the important factors required to maintain good mental health generally. It is a central tenet of this thesis that a positive Affect Balance is one such element. If Affect Balance is to be considered a serious theoretical and clinical concept it is important to ground it in a theory of emotions or of core elements of such a theory, since there is little consensus about Emotion Theories at the present time. Though its value for a range of research tasks needs to be explored, this

chapter is not the place to do this. These matters will be discussed in Chapter Seven after the utility of Affect Balance has been tested empirically.

It remains for me to comment upon Affect Balance as a clinical tool in this research. Its utility can be discerned from a moment's thought concerning its original purpose: namely, the assessment of "happiness" and, by implication, psychological well-being. So, as an evaluative concept in clinical practice it is clearly very relevant.

The experience of the data collection impressed upon me the relevance of using the instrument with clinic attenders. The modified form used requires the client to make judgements about their life-domains which taken together cover most of life's roles and activities. This process alone I found to have extremely valuable therapeutic pay-offs. The most prominent and practical use of this instrument is for the creation of therapeutic agendas which focus on those areas most in need of attention. Moreover, in scrutinizing the form it is relatively easy to determine which areas are within the control of the client and which are not, so that therapeutic effort can be expended in the most propitious ways. My personal experience in using the form with clients, and their response to it, was both satisfying and extremely rewarding.

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CHAPTER 3

DESIGN, DATA ANALYSIS, AND RATIONALE

The design of this study was constrained by two overriding objectives which are embodied in the rationale of the study. As such they exemplify the classic compromise of social science research, between the desire to employ as tight a method as possible in the service of valid and reliable data, and the need to validly sample a social context: the social process in all its complexity. In any social research endeavour, a balance has to be found between what is considered to be the paradigm case of scientific research, the rules and procedures of experimentation, and the practical realities of social life.

The study population was drawn from a mainstream alcoholism treatment agency and the work reported here describes an attempt to establish an ongoing clinical assessment and evaluation procedure for this alcoholism treatment programme. This involves the use of a variety of clinically relevant measures that provide baseline, process and outcome measures. This represents one objective that corresponds to the more conventional alcoholism treatment evaluation aspect of this thesis. Although outcome data are collected which are of practical value for the treatment programme, the work undertaken focuses not so much on treatment as on the recovery process which takes account of the treatment programme.

The second objective is to employ the Affect Balance measure, as both a theoretical and an operational construct as

was described in the previous chapter. At the level of theoretical concept, Affect Balance is presented as an overarching concept which can be utilized both as an independent and a dependent variable. In this evaluation it links an individual's life experiences with the treatment experience, both of which are construed as components of a recovery process. In this context it is to be noted that Affect Balance can profitably be discussed in the micro-macro translation debate in social science methodology. At the "shop floor" level, it is used as a clinical tool, as noted in Chapter Two.

Together, these two concerns steer this evaluation study away from the type of treatment evaluations reported on in Chapter One. It was noted in the literature review that major reviewers commented upon the absence of sound experimentation, particularly adequate sampling, and the randomization to treatment and control groups. Such procedures are necessary to tap the causal determinants of treatment effectiveness. It would seem that it is rarely possible to measure both cause and effect simultaneously in the alcoholism treatment field.

In part the disappointments reported in the treatment outcome literature are responsible for attempts in this study to go back a pace and construct a source model of the recovery process. Relative treatment effectiveness insofar as it is employed at all in this study will simply be judged against norms reported in the literature. The wider focus of this study is very much in the spirit of the second stage of

Tuckfeld's model discussed elsewhere. This theoretical concern is analogous to the "sources of variance" paradigm introduced into psychology by Endler and Hunt (1968) in the persons-situations debate. Unlike that debate, the rationale of this study is not to engage in discussion over the relative degrees of variance attributable to particular factors, as in the spirited polemic which characterized the "Situationism in Psychology" debate (Bowers 1973). Instead, Affect Balance is presented as a way of making sense of the recovery process by emphasizing and summarizing person-situation interactions. An implicit assumption is that such a measure will account for an enhanced degree of outcome variance. In this context, Affect Balance is a descriptive noun, the validity of which is at issue in this study. These two objectives, one practical, the other theoretical, have to be accommodated in the research design that has been employed; little guidance in this regard is to be gleaned from the literature reviewed.

What has been taken from the literature is the need to employ a multi-dimensional frame of reference. In discussing the rationale of this study, I have in essence been describing two disparate kinds of study, one enumerative where the interest is in the estimates of a numerical sort only, the other analytic where interest is in causal processes. Typically, the methods of research design and analysis are different for the two types of study (Deming 1975). Because of the analytic aspect of the study, it is necessarily exegetical and goes beyond simple enumeration in its attempt

to grasp the relationship of individuals' experience in their "small life worlds" and the interactions of such with the recovery process. A more elaborate title for the study might be "A combined exploratory-descriptive process and outcome study of the recovery process in alcoholism treatment for the purposes of theory construction."

SUMMARY OF STUDY AIMS

In addition to the theoretical aims of the thesis mentioned above, the specific empirical aims are:

1. To examine the correlates of extended contact with the treatment programme.
2. To examine the relationship between Affect Balance and other key variables relative to the characteristics of the problem drinkers selected for investigation.
3. To assess the relative value of Affect Balance and other baseline measures in predicting a range of nominated outcome measures including the duration of respondents' contact with the treatment programme.

DESIGN

Before-and-after design with repeated measures has been used with a single group. This design enabled treatment to be monitored over a six-month period by the administration of a battery of tests before and after the six-month period; with a selection of tests from the battery being administered at monthly intervals. The total time-frame from the initial data-collection point is seven months. This strategy provides change scores such that a treatment effect, or at least change within an individual whilst in the treatment programme, can be

measured. It does not allow one confidently to ascribe change due to treatment. It is not a time series since there are no treatment manipulations involved; rather it is a monitoring procedure within a before and after design.

However, the design permits the collection of data which can be analysed to demonstrate the impact of extra-treatment effects or extraneous variables, primarily through the use of the Affect Balance Scale. The value for the design of being able to measure co-variation between extraneous and process variables in the analysis is that it then operates in the manner of a "matched subject design" to accommodate the second study objective: what is controlled is variation in the individuals' subjective evaluations of life experiences and the relation of these to outcomes, instead of variation in the treatment process, such as would normally be the case. Treatment remains constant throughout the study period. Given these objectives, this design procedure modifies and improves upon a straightforward before and after design.

DATA ANALYSIS

The exploratory nature of the study means that little can be assumed. For some measures employed, the psychometric characteristics are unknown. For others, a normal distribution can be expected. Hence, parametric or non-parametric statistics will be used as necessary after scrutiny of the distributions of the study variables. The analysis will make use of the SPSSX/PC package. Descriptive statistics will be obtained for all variables at all phases. Attempts will be made to see if completers who remain in treatment to

the end of the study period can be predicted from baseline measures on summary statistics. The sample will be divided by attrition per phase into WinCodes 1 to 7; in which completers are WinCode 7. The summary statistics have practical value for programme planning.

Changes in mean scores across the study period will be the main device for observing change. The pattern of change is what is of interest both on an enumerative level, and an analytical level. Extraneous variables (e.g. non-treatment variables) can be controlled such that their influence on the pattern of change and outcome can be studied. A secondary analysis will utilize a correlation matrix to see if it is possible to create a drinking X quality of life typology. Pre-eminently the data analysis will follow an exploratory route, utilising analysis of variance and regression where appropriate.

POPULATION

The study-population comprised the entire treatment population of the recovery programme who survived the first induction (survival) phase. Only three individuals, who left at the very beginning due to illness, were missed. There were no refusals to co-operate. Generally, the programme population is thought to be typical of most National Health Service Alcoholism Treatment Units.

DESCRIPTION OF PROGRAMME

The recovery programme is organized on a phase level basis around group work (See diagram below). Its duration was approximately 13 months (now reduced to 6 mths). Referral can

be from any source, the most usual being Hospitals, Social Workers, G.P.'s, self-referral, and the Industrial Alcoholism Unit in Glasgow.

PROGRAMME

Entry:	S. Gp.	Ass. Gp.	Int. P. Gp.	Fl. P. Gp.
Duration:	6 wks.	4 wks.	13 wks.	13 wks.
		B1	P1, P2, P3, P4, P5,	B2.

Legend S. Gp. = Survival group
 Ass. Gp. = Assessment group
 Int. P. Gp. = Intermediate phase group
 Fl. P. Gp. = Final phase group
 B1 - B2 = Initial full assessment battery
 P1 - P5 = Monthly selections from initial battery

After referral to the programme a client will be asked to attend for the first time on either a Tuesday or Thursday evening. They will be welcomed to the clinic by a clinic worker who will have been specially detailed to work with new referrals (This task is undertaken by workers on a monthly rota basis). The new client will have the clinic programme explained to him or her over a cup of coffee in an area of the clinic set up as a kitchen/lounge area.

The task for the worker is twofold:

- A. to explain the when, where and how of the programme to the new arrivals so that the client is quite clear when he or she leaves, what will happen next.
- B. the second function of this initial introduction to the clinic is for the worker to simply sit and listen to the person in front of them. Experience has shown that many such new referrals have a need to speak about themselves and the problems that have brought them to clinic, during this first

session, often very expressively and emotionally. Exactly the same experience applies to relatives who are seen for the first time, perhaps even more so.

This sympathetic listening to the client's or the spouse's view of their situation during the first contact session, is considered to be highly therapeutic. A high level of empathy during this session is the ideal.

If the client returns for the next session of the programme, they will go into the 'survival' group. This is an open ended induction group which lasts six weeks. Its size varies between 10-20 individuals. The purpose of this 'survival' phase of the programme is to allow attenders to 'dry out', before they progress to small therapeutic groups. The 'survival' phase was christened such, by a client in the early days of the programme and the label stuck; clients have to 'survive' the early difficult period of their recovery to progress onwards.

In the large group little pressure is put upon clients to make individual contributions, in many respects it resembles a school room more than it does a therapeutic group. During the survival phase the client will be introduced to the clinic programme, its rationale and a syllabus of various issues to do with alcohol and social education. The sessions, although controlled by a single worker, may be contributed to by a variety of clinic workers. The Survival Group meets three times per week, two evenings and a Saturday morning. As with all the recovery groups there are communal tea or coffee facilities ongoing during the clinic nights which attenders

are encouraged to use free of charge.

Progression onward from the survival group is to the Assessment Group. Here the client will be one of perhaps 6 - 12 individuals. The group has a clinic worker leading it. This assessment group was instituted by the present research project and has become a permanent feature of the clinic programme. Its primary purpose is to collect assessment data from the group members and feed it back to them in a discursive fashion. In addition to providing basic assessment data for evaluative and programme planning needs of the clinic, the data is intended to provide each person with:

- A. an objective overview of their problem and
- B. a personalized agenda for future work.

Assessment group sessions have a flexible format to accommodate data collection but attempts are made to ensure that a significant proportion of time is spent on group discussions. The group meets twice a week and lasts 4 - 6 weeks depending on the numbers and availability of follow on groups or workers.

After completion of the assessment group, the client moves into a mid-stage group, which is a small closed therapeutic group of 8 - 10 individuals. These mid stage groups lasted originally for six months. They have now been reduced to three months duration - mid stage groups are run by group workers and the style and content of the work undertaken is that of a small problem-centred group. It is hoped that by this stage in the recovery process individuals have acquired

enough energy and motivation not to devote all the group time to ruminating about drinking or not drinking.

On completing the mid stage group the client moves into the final stage group. Again this group lasts for three months, having been cut from six months, like the mid stage group. This is a small (6 - 10 persons) closed group where group interaction is focused upon experiential issues and discussions of matters of lifestyle.

Both the mid and final stage groups meet once a week. Generally group work within the programme is informed by a social education model rather than a disease model of alcohol problems.

PROGRAMME STAFF

Three full-time staff are involved: two consultant grade psychiatrists, and a social worker. These individuals all contribute evening and Saturday morning sessions as part of their full-time contracts. By far the most numerous staff members are volunteers, 25 in total. The majority of these volunteers are programme "graduates" themselves, and they come from a variety of occupational backgrounds. The premises are owned and controlled by the Greater Glasgow Health Board. Volunteers are acknowledged formally by NHS management and are reimbursed their travelling expenses.

INSTRUMENTATION

The instrumentation falls into three conceptual blocks:

- A. Drinking Profile
- B. Psychological Profile
- C. Social Integration.

A. The drinking profile comprises three separate instruments:

1. The Alcohol Use Inventory

(Source Wanberg K.W, Horn J.L and Foster F.M. 1977)

This is the primary alcohol assessment instrument used in the study. This instrument developed through a process of factor analysis and item-rewrites from the 'Drinking History Questionnaire' - a set of 147 items emerged. The final version was based on a sample of 1030 individuals at the Fort Logan Mental Health Centre, Denver, between 1969 and 1971. Subsequent work on the validation of the scales was carried out by the authors up until 1976. A reference group of 2261 patients was used to provide norms. This group did not differ from 4,500 patients admitted to the Fort Logan facility from 1970 to 1975. Adequate internal consistency, independence of scales, and reliability is claimed for the scales. There are 16 primary scales, and 4 second order scales; and a summary scale consisting of the maximum of variance of the other second order scales. This latter scale is labelled the General Alcoholism Scale. The items of the AUI cover three conceptually distinct domains.

1. Styles of alcohol use.
2. Symptoms causing distress.
3. Benefits from drinking.

The results are plotted onto a graphic profile summary where an individual's scores can be compared with reference group norms. The respondent reads the items from a booklet, and gives his answers on a separate answer sheet. This is then scored by making use of four plastic overlays. It can be administered either singly or to groups. Administration, scoring and profiling are clerical tasks. The test takes approximately 20 minutes for a respondent to complete.

The names of the 16 primary scales are as follows :-

1. Social Benefit Drinking. 2. Mental Benefit Drinking.
3. Gregarious Drinking Style (Bi-polar scale : low = isolate)
4. Obsessive-Compulsive Drinking.
5. Sustained vs. Periodic Pattern Drinking (Bi-polar scale)
6. Post Drinking Worry, and Guilt.
7. Drinking to Change Mood.
8. Prior use of External Help to Stop Drinking.
9. Loss of Control when Drinking.
10. Social Role Maladaptation.
11. Psychoperceptual Withdrawal (D.T's)
12. Psychophysical Withdrawal (hangover)
13. Use of Other Drugs. 14. Quantity of Alcohol Used.
15. Drinking followed by Marital Problems.
16. Drinking Provokes Marital Problems.

The second order scales are :-

Dimension A Self-Enhancing Drinking.

Dimension B Obsessive-Sustained Drinking.

Dimension C Anxiety-related Drinking.

Dimension D1 Alcoholic Deterioration.

Dimension D2 Alcoholic Deterioration (adjunct).

Dimension G General Alcoholism.

D1 and D2 correlate at 0.77 and therefore D2 can be used as a validity measure. The AUI profile provides individualized information on symptoms, benefits, behaviours and styles of alcohol use, which can lead to treatment planning.

Certain difficulties arise in using the AUI as an outcome measure in a primarily abstinence-orientated programme. Whereas the AUI is an extremely useful instrument for assessment with a drinking or recently drinking population, it becomes either inappropriate or very liable to distortion with a population who have remained in a recovery programme for a number of months. The AUI is not then a valid outcome measure in this study.

2. DRINKING-RELATED LOCUS OF CONTROL (D.R.I.E.)

(Source : Donovan and O'Leary (1978)).

Confusing results using a general locus of control measure with alcoholics led to the development of the topic-specific, drinking-related locus of control scale, which translates generalized expectancies of control into a measure of specific expectancies dealing with a variety of drinking-related behaviours. The scale consists of 25 items in a

forced choice format. Each item consists of a pair of statements, one indicative of internal control, the other of external control. The order of pairings is reversed after ten items to combat method effects!

The DRIE scale correlates significantly with Rotter's I - E scale (Rotter 1966). Four scores are derived from the items:

1. Total score - all items endorsed.
2. Intrapersonal control - 7 items.
3. Interpersonal control - 7 items.
4. General control - 3 items.

Donovan and O'Leary (1978) investigated the psychometric properties of this scale by, amongst other methods, comparing it with AUI profiles. They conclude that the scale demonstrated convergent, discriminant and concurrent validity, as well as construct validity. Control orientation to drinking situations is a commonsense variable to measure before, during, and after treatment. In this study only the first total score (all items) will be used, since inter and intra-personal control is measured in the AUI scales. Amongst others, Walker et.al. (1979) have shown that DRIE scores should decrease with treatment, and also that it is a predictor of attrition (Walker et.al. 1980).

3. SEVERITY OF ALCOHOL DEPENDENCE QUESTIONNAIRE (SADQ)
(Source : Stockwell et al (1979))

This is a 20 item self-report questionnaire. The respondent is asked to focus upon a recent month of typical drinking, or a similar period suitable to the task in hand.

The form of the questionnaire was changed from the original 1979 version by the deletion of 3 items. There are now 5 sections comprised of four items each, these are :-

1. Physical withdrawal signs (PHYS)
2. Withdrawal relief drinking (NEED)
3. Affective withdrawal signs (AFF)
4. Quantity and freq. of alc. consumption (ALC)
5. Rapid reinstatement of withdrawal
after abstinence (POSTAB)

Each item is rated on a 4-point frequency scale and scored 0,1,2,3 accordingly. The range of scores is therefore 0 - 60. The cut-off point for severe dependence is 30 and 29 or less for mild to moderate dependence. (These cut-off points misclassify 18% of respondents) The SADQ is one of a number of similar instruments and is probably the most well-known and widely used.

Norms are provided from three different populations as an aid to interpretation by the authors. Good evidence is presented for reliability, construct and concurrent validities. It is a quick, reliable and valid assessment instrument for gauging the degree of alcohol dependence.

Severity of dependence is generally acknowledged to be an important clinical concept which, in turn can be of importance in treatment planning. It helps to separate out high alcohol consumption from alcohol dependence. This is an important consideration when deciding whether an individual should attempt total abstinence or try to control his drinking. Other issues include the assessment of the many

problems an individual may have, such as phobic anxiety which are associated with dependence upon alcohol and frequently disappear when the person stops drinking. Measuring severity of dependence can save the clinician, and those on the treatment programme valuable time and resources by helping to avoid the misdirection of clients into inappropriate therapies (Hodgson and Stockwell 1985).

These three instruments, the AUI, DRIE and SADQ complete that part of the battery concerned with the individual's drinking profile. Taken together, the three instruments provide a fairly comprehensive profile of a persons' drinking problem.

B. The next section of the test battery is concerned with a person's psychological profile. Here again three separate instruments are utilized. In turn, these are :-

1. The Crown-Crisp Experiential Index.
2. The Gough Adjective Check-List.
3. The Coopersmith Self-Esteem Inventory.

1. THE CROWN - CRISP EXPERIENTIAL INDEX (CCEI)

(Source: Crowns' Crisp A.H. (1979))

The CCEI, (previously known as the Middlesex Hospital Questionnaire (MHQ), (Crown-Crisp 1970) is in a self-report format from which subjects pick the answer that best applies to them. It consists of 48 questions phrased in ordinary language designed to elicit symptoms and traits indicative of the conventional categories of psychoneurotic illness. It provides the sort of information that would normally be gleaned from a formal, clinical psychiatric examination. It

is easily understood and completed by respondents and it can be completed either individually or in groups and takes approx. 10 minutes to complete. It is scored with the help of a plastic overlay. The authors list three uses for the instrument, These are :-

1. Personality profiles.
2. Screening.
3. Measurement of change.

The measure yields a profile of six sub-scales which can be graphically represented against scale-norms. The sub-scales are :-

- | | |
|--------------------------------|--------------------------|
| 1. Free Floating Anxiety Scale | 2. Phobic Anxiety scale |
| 3. Obsessionality scale | 4. Somatic concern scale |
| 5. Depression scale | 6. Hysteria scale |

Data are presented on the validity and reliability which have accumulated over a period of years. Also, normative United Kingdom data are available for comparison and interpretation purposes. Five reference groups are available for comparison:

1. Urban males
2. Urban females
3. Rural males
4. Rural females
5. In-patients with psycho-neurotic illness.

The sub-scales are conceived of as "Normal" psychiatric descriptions of personality traits; if the scores are high enough, they are regarded as symptoms.

The psychometric credentials of this test are well-established except for the final sub-scale, Hysteria. It seems likely that this sub-scale taps elements of extroversion. In practice, it would have been more difficult

to exclude the HYS scale from the test than to include it.

2, THE COOPERSMITH SELF - ESTEEM INVENTORY (SEI)

(Source : Coopersmith 1967 and 1981)

There are three forms of the SEI, two for schoolchildren and one for adults. The adult form "C" is used here. It is for use with persons over the age of 16 years and is derived from the short school form "B". The psychometric properties of the SEI have in the main been evaluated from the school forms.

The adult form "C" is an adaptation of the school forms where the wording of items have been changed to make it suitable for persons over the age of sixteen years. Form "C" has 25 items with a forced choice format, in the form either "like me" or "unlike me". The form is simple to complete and only takes a few minutes to complete. It is scored by template, counting the positive responses.

The issue of self-esteem raises conflicting views of the construct as either a unitary or a multi-dimensional phenomenon. The Coopersmith SEI essentially treats self-esteem in a unitary fashion, although it is pointed out in the manual that various domains are tapped. The adult form "C" addresses peer group, family, and work but most of the 25 items are self-referential. My own preference is for a multi-dimensional scale, but the psychometric measurement issues involved in the production of multi-dimensional scales are such that the numbers of items necessary for such a scale would greatly expand the size of the instrument. As such it would have been unsuitable for the present battery of tests

which is already quite extensive. Initially, when I looked around to see what instruments were available, I was, frankly, unimpressed with the limited choice and quality on offer. I chose the Coopersmith scale not because I considered it to be the best. In fact there did not appear to be much to choose from, or indeed much of a difference between the best and the worst. I made my choice of the Coopersmith rather, because of the simplicity and ease of scoring. However, although this scale does not tap the conceptual richness and variety of self-esteem, this is not to say that it cannot do a workmanlike job for the task in hand. With regard to the ongoing assessment process, I intend to replace it with a more clinically useful multi-dimensional measure.

The self-esteem measure completed that part of the assessment battery concerned with psychological state.

C. The next conceptually distinct section of the assessment, are those measures concerned with social integration. There are five measures in this section:-

1. The Social Behaviour Assessment Schedule (modified).
2. The Interview Schedule for Social Interaction.
3. The Affect Balance scale.
4. Brief social adjustment scale.
5. Life event inventory.

1. THE SOCIAL BEHAVIOUR ASSESSMENT SCHEDULE (SBAS)

(Sources : Platt et.al. (1978,1980)).

The original SBAS assessed patients' psycho-social behaviour from the perspective of role-domains and experienced symptomatology. The patient's actual behaviour is described

in a number of role-domains on a 3-point scale i.e. full performance, impaired performance and no performance of the role. The format is a semi-structured interview during which the interviewer has to make assessments with the aid of objective criteria, of the extent to which the individual falls short on any particular item describing the role. What is unique about this instrument as opposed to many others is that although an assessment is obtained from the respondent (who is a relative or close friend of the patient) about the relative occupation of roles and symptoms, no inference about the meaning of the behaviour for those concerned is made. The ratings are purely descriptive. A second distress scale measures the informant's emotional reaction to any disturbance. A third scale measures the onset of the disturbance.

For the purposes of this study, I re-wrote three of the scales into a format suitable for completions by the client. These were, the Disturbed Behaviour Scale that comprised 21 items asking about things like being indecisive, irritable, suicide attempts, odd ideas and so on. These were rated as 0 = none, 1 = moderate, 2 = severe, 3 = resignation (zero-value), and 9 = not applicable. The next scale used was the Social Role Performance Scale which comprised 12 items, and considered things like housework, work/study, spare time activities etc. Again these had a presence and distress (or burden) score as above. The last scale was the social support scale which asked about support from relatives, friends, neighbours and from social welfare agencies. These were rated

from actual contact, relief from such contact, practical help, and relief from practical help. A final section on the modified SBAS enquired about housing situation, and length of time in the present house and in the district.

The answers were transferred to a specially constructed score sheet and summed to provide scale totals. This instrument takes about 35 minutes to complete and is time-consuming to score. It also accounts for a considerable amount of bulk in the test battery booklet and, although it suits the purposes of this research, I have decided to drop it from the ongoing assessment because of its size.

Essentially, what is tapped by this modified version of the SBAS is a range of symptoms, social role performance and social support. Since the modified version of the SBAS departs in a significant way from the original, none of the validity and reliability claims made for the original can hold with my version. However, a claim can be made for face validity since the items are clear and unambiguous and experience with its use does not suggest that it is at all problematic for the respondent.

2. THE INTERVIEW SCHEDULE FOR SOCIAL INTERACTION

(Source : Henderson et.al. (1981))

Based upon Bowlby's theory of attachment, this instrument was developed in Australia and examines the immediate social environment of the respondent. This is done by a systematic examination of the individual's "primary group". The interview schedule consists of 61 questions from which 4 main scores are derived, together with supplementary

scores. These are :-

1. AVAT - Availability of attachments
2. ADAT - Perceived adequacy of attachment
3. AVSI - Availability of social integration
4. ADSI - Adequacy of social integration

Since the Adequacy of attachment scale (ADAT), is logically dependent upon the AVAT scale, the availability of such attachments, there is a variability of answers for the ADAT scale. The ADAT% score simply divides the adequacy ADAT by the availability AVAT score, and provides a score figure for comparisons. For those individuals who do not have any attachments, their NONAT score gives a measure of their satisfaction, or lack of it, with this state of affairs. The schedule takes about 30 minutes to complete, and was hand-scored by transferring answers to a constructed answer sheet, sub-divided into scales. Each item was answered 0 - 1. Extensive data are provided on the reliability and validity of this measure. Although it is an extremely long instrument to use, and somewhat tedious to score, it seems to provide little difficulty for respondents.

It was necessary to inflict a minor violation on the protocol in the course of administration. The authors advise that the form should be used in an interview situation. However, in the research presented here, it was administered in a group situation with an interviewer(group worker) in attendance. This sort of administration appears to have been satisfactory. The norms provided for the Australian sample in Henderson et.al. (1981) which summarizes most of the

development work reported in journal articles, will have some relevance as a standard of comparison.

3. THE DOMAIN AFFECT BALANCE SCALE (DABS)

The previous chapter introduced the concept of Affect Balance together with Bradburn's Affect Balance Scale which, it will be remembered, was used for large-scale survey work. Bradburn's scale was a ten item measure with five positively worded items labelled the Positive Affect Scale, and five negatively worded items, the Negative Affect Scale. The items were of high level of generality both in terms of the feeling-state tapped, and its domain. Since the scores on these two scales were found to be independent, a third overall score, or Balance Score, was derived by subtracting one from the other. This latter was labelled the Affect Balance Scale.

The items in the scale describe mood states which, logically, have referents in the world. Taken together, they describe the person's feelings about their life which, it is hypothesised, have a major bearing upon mental and physical health. In this way, quality of life is seen as an important variable for mental health. A method of assessing quality of life is to imagine an individual's life as being comprised of a variety of life-domains which cumulatively describe the person's total life experience. It is at this level that I designed a scale to measure Affect Balance. For reasons given in the previous chapter, I have decided to retain the underlying balance model, although there have been criticisms made concerning its utility. Future research may well dispel the theoretical independence of positive and negative affect.

If this were to happen, then one will at that point, simply be dealing with unitary judgements about life-quality.

I am heavily indebted to the Quality of Life literature for ideas about the Affect Balance scale to be described, particularly, to the work of Andrews and Withey (1976) who took as a starting point the difficulties in measurement that were entailed in the interaction of subjective perception with objective life-circumstances. They studied Americans' perception of their quality of life. This work, amongst other things, demonstrated how Americans tend to feel about different aspects of life, and how these evaluations combine to produce a feeling about the overall value of life. They produce a model which describes well-being at different levels of experience. The most expansive is well-being at the global level. This is equivalent to Bradburn's "life as a whole". Next, the function of the model is to specify how the "life as a whole" evaluation is arrived at and, what particular concerns make up the global or absolute perspective. The next level of the model specifies a range of "concerns". Their research involved testing out series of "concerns", that is, life-concerns on things that impinge upon a persons sense of well-being, like the state of the National Government, or the state of one's immediate neighbourhood, or house, or family, or work and so on. These can then be tested against the global evaluation to see how much variance they account for. The research task was to account for as much of this variance as was possible. Correlation with life as a whole then, was the criterion for

the inclusion of items into this measure. They ended up with 13 concerns highly correlated with the global measure. These 13 concerns were measured by 132 items. Together, these 13 concerns accounted for 50 - 60% of the overall variance of feelings about life as a whole. This is a very high percentage when one considers that many relevant psychological data are not tapped at all by these measures. The concerns level of the model is differentiated into domains and criteria. The domains are the concern labels, or subject of the evaluations like housing, leisure, work etc., and the criteria are what the domains are evaluating. i.e. success, independence, beauty, safety, fun. The criteria then are personalized values by which an individual assesses his/her life-domains.

It is from the list of domains that I have chosen the pool of life domains for the present measure, fitting them then into a Bradburn-type balance model. The actual number of domains measured is logically dependent upon how broadly or otherwise each domain is defined. For instance, in my scale, Family Life and Marriage, are two separate domains because of the clinical expediency of making this distinction, but, Family life could just as easily be defined such that it included Marriage. Retirement, an obvious life-domain, is not included in the present scale because the population studied was not expected to include many pensioners.

The measurement format of this scale is also heavily influenced by the work of Andrews and Withey. Having tested out a variety of response styles, one they recommended was

the delighted-terrible scale. This is a 7-point scale. The point is made by the authors that a 7-point scale is about as fine a judgement as people can make about most tasks. The scale is labelled as follows :-

DELIGHTED	PLEASED	*M.SAT.	MIXED	*M.DISSAT.	UNHAPPY	TERRIBLE
7	6	5	4	3	2	1

(*=mostly satisfied: and, mostly dissatisfied)

In the Andrews and Withey scale 3 alternative response modes are available:

A. Neutral B. Never thought about it C. Does not apply.

The Life-Domains Affect Balance Scale incorporates the final option C, "does not apply", but not A and B. It also incorporates a visual analogue scale to measure control, satisfaction and importance. These measures have conceptual, clinical and psychometric virtues; they were measured on a 10 cm. line.

Control is a theoretically important variable and offers a theoretically different way of measuring the construct domain. Importance offers the possibility of rank-ordering the domain such that finer discriminations can be made, possibly with the option of weighting the domain according to importance. Satisfaction is conceptually different from happiness or delight. Research on subjective social indicators has consistently shown that young people are frequently happy, whilst still being dissatisfied (Andrews and Withey 1976). This seems to turn on issues concerning life aspirations. The list of domains measured are as follows :-

- | | |
|------------------------|----------------------|
| 1. Family life | 8. Friendship |
| 2. Marriage | 9. Housing |
| 3. Neighbourhood | 10. Religion |
| 4. Parents | 11. Leisure |
| 5. Work/unempl. | 12. Organizations |
| 6. Financial situation | 13. Legal situations |
| 7. Health | |

These 13 domains are thought to encapsulate the daily life and role performances of the study population. Number 13, legal situation, might at first glance appear to be a domain which does not intrude much into the consciousness of the average person, but was included here on the grounds that many of the study population will be involved in ongoing legal entanglements, or will recently have been so as a consequence of their drinking.

The response format is the same for each domain. At the end of the rating of domains Bradburn's global rating is included as the final item on the scale. The question is asked "Taking all things together, how would you say things are these days?"

- (A.) Not too happy
- (B.) Pretty happy
- (C.) Very happy.

This global rating gives one the option of evaluating the relevance of the 13 domains in terms of amount of variance accounted for with that study-population; also it gives us at a glance the state of affairs in respect of avowed happiness, and has therefore clinical utility. Scoring is done simply by summing the positive and the negative responses, taking the

mid-point at (mixed) zero and dividing the total, by the number of domains enclosed.

EXAMPLE OF 1st. DOMAIN

Family life.

Delighted Pleased M.Sat Mixed M.Unsat. Unhappy Terrible

does not apply []

do not feel in control	-----	feel completely in control
very unimportant	-----	very important
very unsatisfied	-----	very satisfied

During administration there was guidance about using the visual analogue scale. Generally this measure presented no problems in either individual or group administrations as clear written instructions were provided.

The Life Domain Affect Balance scale completes the list of instruments used in the main test batteries. Three other measures were included in the process measures.

5. A LIFE EVENT INVENTORY

(Source Tausig 1982)

This is a list of 107 events endorsed for desirability, namely, Desirable versus Undesirable; Expectation, i.e. expected "yes" or "no", and Importance rated not important, moderately important, and very important. This is a modified version of a Life Event Inventory produced by Tausig (1982).

5. THE BRIEF SOCIAL ADJUSTMENT SCALE (BSAS)

This scale is a modification of the social adjustment scale used by Pomerleau and Atkins (1980). This was itself a modification of the social adjustment scale of Weissman, Paykel, Siegel and Klerman (1971). It is a seven item scale which asks respondents to rate on a 5-point scale the 7 domains covered by the scale, which are:-

1. Job 2. Social life 3. Close friends 4. Spouse
5. Children 6. Parents 7. Recovery programme

This is exactly the same as the Pomerleau scale except for the last item which, in the Pomerleau scale enquires about time lost from work, housework, or school. The inclusion of the item asking about the individual's performance on the programme has practical application for the programme monitoring procedures.

The Brief Social Adjustment Scale was not included in the initial assessment battery. It was designed for the process measures. The reason for this, apart from the inappropriateness of the last item of the scale for an initial assessment measure, was because the battery already included an extensive scale with considerable overlap, the Interview Schedule for Social Interaction, which was described above. What was required for the process measures which, it will be remembered, were collected at monthly intervals, was a brief check on the domains listed in the scale. There is a wide variety of social adjustment scales available, something in excess of 30 in the literature. However, most of them are considerably longer than the brief scale used here and for

that reason were not particularly suited for this study.

The responses to this five point scale are summed and divided by the number of domains endorsed. If 7 domains were endorsed, the range would be $7 \times 5 = 35$; since not every respondent would endorse all domains, a percentage score was used.

Also included in the monthly process rating were three questions that asked about the informant's experience of the clinic programme that month. A visual analogue scale (10 cm.) was used to rate if it was:

1. relevant
2. interesting
3. helpful.

These were all bi-polar

Finally, 3 questions were asked about drinking:

1. Have you drunk alcohol in the last month? yes/no
2. If so, how many days were you drinking.....
3. Approx. how much alcohol did you consume each day.

Two of these process measures gave acute cause for concern. I was very much aware that the long life events scale was not treated seriously by many of those who completed it. Its validity therefore is extremely questionable; generally, my feeling was that it tended to be ignored by many people. The other measure which tended not to work was the self-report of drinking. Because of my clinical involvement with certain individuals in the programme I know as a matter of fact that on occasions, drinking episodes were not reported. Indeed, my feeling is that the individuals who

reported lapses are likely to be over-represented in the good outcome group. This would indicate that their disclosure was a measure of their authenticity in working in the programme.

Chapter 3 References

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CHAPTER 4.

DATA ANALYSIS I

The purpose of this chapter is to describe the sample characteristics. Starting with a table of the numbers in each phase and their demographic features, all the study-variables across the three conceptual domains of drinking, psychological, and social characteristics will be described. Where appropriate, comparison of the total sample (N = 166) will be made with the group of completers (N = 61).

The data analysis used a combination of data bases and SPSSX/PC for file manipulation and statistical computation. The seven phases B1 to B2 were recorded as WinCode 1 - 7.

Table No. 1

Number of Clients in each WinCode. (N = 166)

Orig. phase label.	WinCode.	N.	%	cum %
B1	1	41	24.7	24.7
P1	2	20	12.0	36.7
P2	3	22	13.3	50.0
P3	4	10	6.0	56.0
P4	5	7	4.2	60.02
P5	6	5	3.0	63.3
B2	7	61	36.7	100.0

This table displays the attrition rate across the study period by phase, (Cum.% column). The table indicates that 50% of the subjects had dropped out of treatment by the end of the third month of the study period. Thereafter drop-out diminished considerably. By the sixth month 63.3% had

dropped out leaving 37%: 61 individuals who completed treatment to the end of the study period. This early period of sharp drop-out rate is a commonly reported feature of alcoholism treatment programmes. An instance of the rule of one third can be observed, in the outcome percentage above. So, at the outset it is possible to observe that the present study reports an outcome rate similar to others that were discussed in the literature review.

AGE Table No. 2

Categories	W.C.All	%	W.C.7	%
1 16-29	37	22.3	9	14.8
2 30-39	54	32.5	17	27.9
3 40-49	54	32.5	26	42.6
4 50-60+	21	12.7	9	14.8

Table No. 3

Age (collapsed into two categories)

	W.C. 1-6	%	W.C.7	%
Under 40 yrs.	65	61.9	26	42.4
Over 40 yrs.	40	38.1	35	57.4 *

* $P = < 0.05$ 1.DF

At the beginning of the study period 91 individuals (54.8) were under 40 years of age, by the end period this proportion had dropped to 42.6 (26 individuals). Conversely, the proportion of those aged 40 and over account for 42.4% (75 individuals) at the beginning of the period but by WinCode 7 this group had increased in proportion to 57.4% (35 individuals). These shifts in proportion achieve significance at < 0.05 level.

Table No. 4

By Sex

	Wincode all	%	W.C.1-6	%	W.C. 7	%
Males	134	80.7	85	81.0	49	80.3
Females	32	19.3	20	19.0	12	19.7
	N=166	100.0	105	100.0	61	100.0

The sample is heavily biased in favour of males. Two Glaswegian samples of recent date, representative of an N.H.S. treatment unit, and a Council on Alcoholism (Allen 1989) reported the proportion of females to males as being 32% and 30% respectively. A proportion of at least 25% might have been expected, given the fairly large size of the sample at the beginning of the study period. The bias is assumed to be a peculiarity of the clinic referral system. There are no statistically significant shifts in proportion between Wincode 1 and 7 for men and women.

Table No. 5

By Marital Status

Cat	W.C. all	%	W.C. 7	%
1.Sing.	33	19.9	11	18.0
2.Mar.	89	53.6	37	60.7
3.D/S	44	26.5	13	21.3
	N= 166	100.0	61	100.0

D/S = Divorced/Separated

Table No. 6

By Marriage Against Other

Cat	W.C. 1-6	%	W.C. 7	%
Mar.	52	49.5	37	60.7
other	53	50.5	24	39.3

	N= 105	100.0	61	100.0

Slightly over half of the sample were married at the beginning of the study period; the proportion accounted for by Wincode 7 did not achieve statistical significance at the 0.05 level. The table was collapsed so as to indicate only two categories, married and other, and a chi-squared test was then done on this data. No significant differences emerged. The proportion of married is similar to other Scottish samples (Kershaw 1973. 54%), (Fischer 1976. 54%) and (Allen 1989. 44%).

Table No. 7

EMPLOYMENT

	W.C.all	%	W.C. 1-6	%	W.C. 7	%
Employed	100	60.2	68	64.8	32	52.5
Unempl.	66	39.8	37	35.2	29	47.5 *
	N= 166	100.0	105	100.0	61	100.0

* $P < 0.05$

The sample is biased towards the employed, slightly more so than the Glaswegian samples reported in the literature: (Fischer 1976.50%) and (Allen 1989. 36%). There were no major differences in the observed proportions between the total sample and the Wincode 7 group, at a statistically

significant level. In respect of this sample at least it does not appear that being employed is, of itself, a crucial characteristic of belonging to the good outcome group at the beginning of the study period. Indeed, the opposite would appear to hold since the proportion of employed individuals has decreased by 12.3% when the Wincode 7 group are compared with the rest of the sample. This difference achieved significance at the 0.05 level.

Table No. 8

SOCIAL CLASS

Cat.	W.C. 1	%	W.C. 1-6	%	W.C. 7	%
1	3	1.8	1	1.0	2	3.5
2	12	7.3	9	8.6	3	4.9
3	41	24.7	27	25.7	14	23.0
4	20	12.0	14	13.3	5	8.2
5	90	54.2	54	51.4	37	60.6
N = 166		100.0	N=105		61	100.0

Social class five predominates here. It includes 10 women who described themselves as housewives. No less than 65 individuals in this category were unemployed. At Wincode 7 there were 4 housewives in category five. The social class distribution mirrors neither the general population (Office of Population Censuses and Surveys 1980) nor published accounts of other Scottish Alcoholism treatment samples because of this over-representation of social class five. Relevant comparisons are listed below.

Table No. 9

	KERSHAW (1973)	WALTON (1966)	FISCHER (1976)	THIS SAMPLE
Cat.	%	%	%	%
1	6	10	9	2
2	15	26	14	7
3	30	36	41	25
4	24	20	22	12
5	24	6	12	54

The two recent Glaswegian samples reported by Allen (1989) do not list a table for the social class of the two samples, but she comments that both of them reflect a social class distribution as found in the general population. The over-representation of category five in the present sample is extremely high. The under-representation in the sample of categories 1 and 2 is also quite marked. It is to be noted that the proportion of social class five increases by WinCode 7.

A probable reason for the social class imbalance in this sample is that a high proportion of referrals to the clinic's recovery programme came from an Industrial Alcoholism Unit who refer a large number of unskilled and semi-skilled local authority employees. These tend to be men employed in labouring occupations, the majority of whom have intact marriages.

Table No. 10

PREVIOUS TREATMENT

	W.C.all	%	W.C. 1-6	%	W.C. 7	%
1 yes	54	32.5	31	29.5	23	37.7
2 no	112	67.5	74	70.5	38	62.3
TOTAL	166	100	105	100	61	100

No attempt was made to assess this characteristic very accurately; the figures result from a single question asking individuals if they had any previous experience of treatment. The responses almost certainly exclude informal types of treatment such as advice and encouragement to abstain, from general practitioners, social workers and interested others. It is notoriously difficult to determine what constitutes treatment in the first place, and then to track individuals across the very extensive range of treatment options available in a city the size of Glasgow. Although most who answered "no" to the item about previous treatment will not have had a referral to a formal treatment agency, they probably have had exposure to a good deal of informal treatment influences from G.P.'s and others concerned with their well-being. In the samples reported on by Allen (1989), 28% reported no previous treatment contact. Fischer reported 25% of his sample having had no previous treatment. Both of these studies used an extensive list of both formal and informal treatment options. Probably, if a similar list had had been presented to the present sample, a much higher proportion would have acknowledged the receipt of some form of prior treatment.

Table No. 11

PROBLEM DEFINITION

	WinCode all			W.C. 1-6			W.C. 7		
G-scale	\bar{X}	S.D.	Range	\bar{X}	S.D.	Range	\bar{X}	S.D.	Range
(AUI)	32.6	13.9	2-67	33.9	14.1	4-67	30.3	13.3	2-53
SevD (SADQ)	28.8	12.8	2-57	28.9	12.3	2-57	28.6	13.6	5-54

	N = 166			N = 105			N = 61		

The scores presented here are shown so as to define the problem status of the sample, as it were. They are rather low for a treatment sample, particularly the General Alcoholism Score (G Scale) of the Alcohol Use Inventory (AUI), which places the sample in the fourth decile range of this instrument's standardization sample (comprised of attenders at an American treatment centre). That is, 60% of the American sample had scores the same as, or greater than the present sample. It is therefore located in the category of low to medium range scores. The distribution of scores on the G-scale were :-

Low	= 38 (22.9)
low to medium	= 58 (34.9)
high medium	= 50 (30.1)
high	= 20 (12.0)

Less than half (42%), of the sample fall within the high medium to high range of this measure. A somewhat lower proportion than might have been anticipated of a treatment sample.

The SADQ score places the sample in the high moderate category of severity of alcohol dependence; a score of 30 or

above indicates severe dependence. The standard deviation of 12.77 is not large in comparison with the measure's norms provided by the authors (Stockwell et. al. 1983), therefore the categorization as high moderate as opposed to severe dependence appears safe since it is not possible to explain the mean being lower than perhaps expected, by indicating a wide range of scores. The scores were grouped as follows :-

3 - 20 = 38 (22.9)

21 - 30 = 49 (29.5)

31 - 59 = 79 (47.6)

When compared with Allen's (1989) recent Glasgow samples, the SADQ scores appear to be high. She reports mean SADQ scores of 22.4 with a standard deviation of 13.2 for both of her samples combined and 27.1 (S.D. = 14.7) for her A.T.U. subset which included the most severe cases. Only 30% of Allen's samples scored above 30, compared with 47% of the present sample.

The inference to be drawn from a scrutiny of these scores is that the sample does not exhibit excessively high proportions of severe alcohol dependence, or more generalized problems associated with such a dependence. It is not possible to differentiate completers (Wincode 7s) on the two measures at the beginning phase.

SUMMARY OF SAMPLE DEMOGRAPHIC CHARACTERISTICS

The initial sample (N = 166) has been described by demographic characteristics and problem definition. Comparisons with relevant samples reported in the literature were noted where appropriate in order to determine whether

there were any obvious peculiarities in the current sample that might require elucidation and explanation. A bias towards low social class and one in favour of males was observed and commented upon. Comparisons were also made with the scores for the group of study period completers, WinCode 7s, where appropriate, to determine if there was any possibility of deriving a separate WinCode 7 profile. Differences of age, proportion of marrieds and of employment were observed.

The sample consists mainly of employed, social class five, married men who are under 40 years of age, with no prior experience of formal treatment and with mild to moderate alcohol problems. The remainder tend to be unemployed and not living with a spouse; these may have had previous experience of treatment.

DRINKING CHARACTERISTICS OF SAMPLE

THE ALCOHOL USE INVENTORY

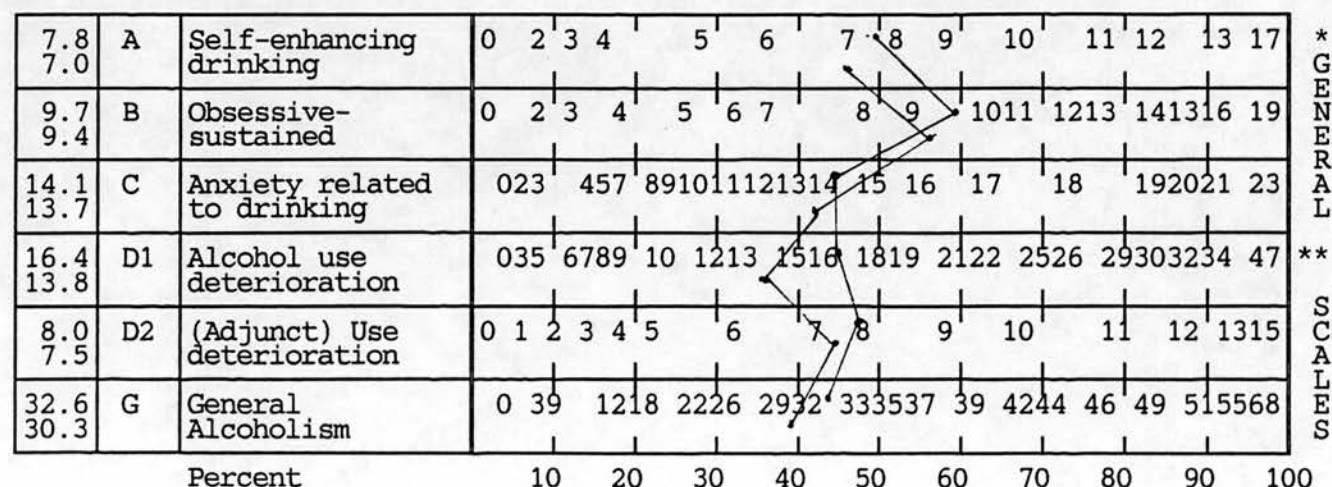
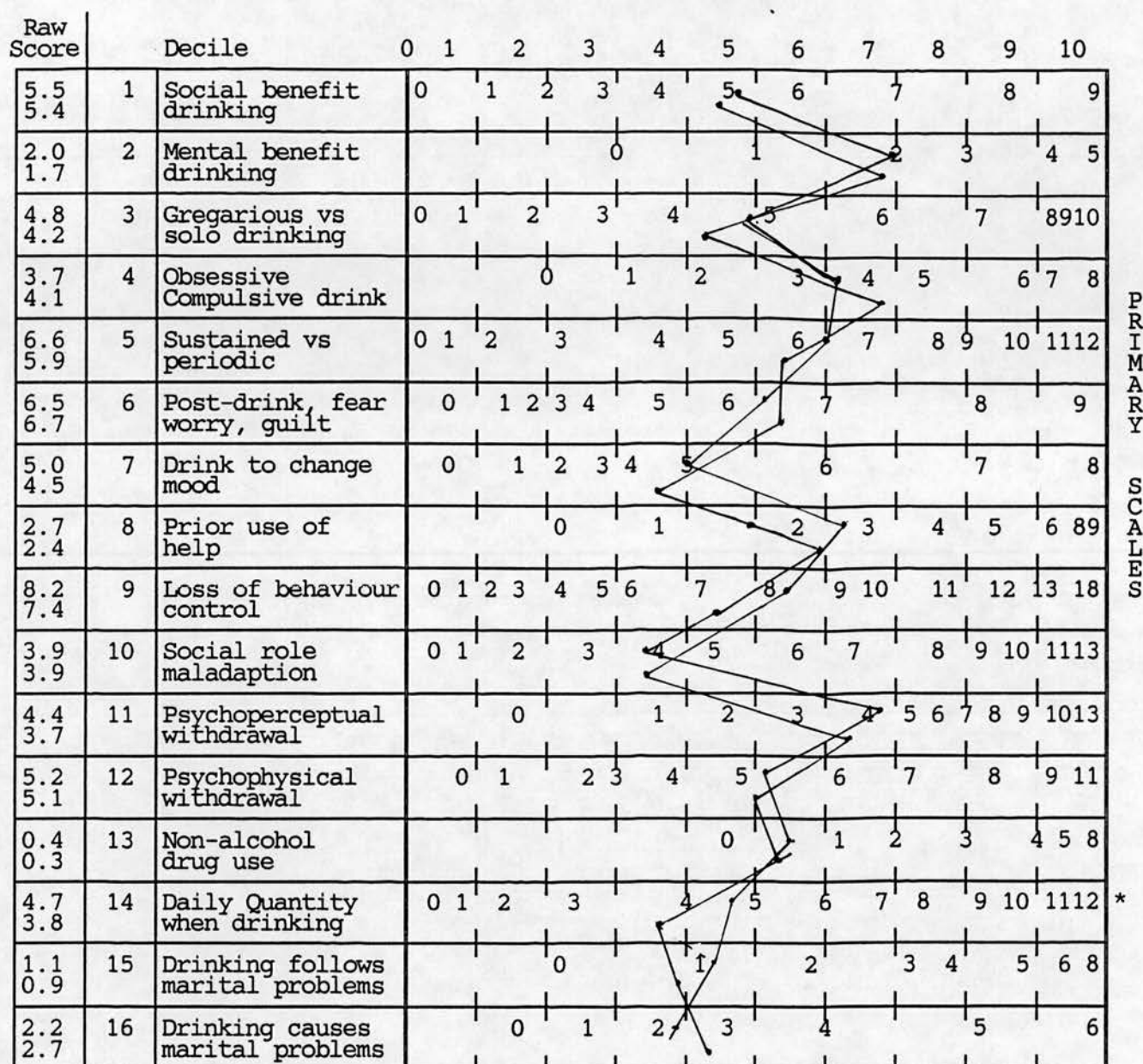
Description of Drinking Variables are shown overleaf, raw scores are converted into labelled deciles categorized as :-

1. low 2. low-medium 3. high-medium 4. high

The score is simply read off the scale at the top and bottom of the profile sheet for each variable or dimension.

ALCOHOL USE INVENTORY PROFILE

For Wincode 1 - 6 and Wincode 7
(Top figure = Wincode 1 - 6 Lower figure = Wincode 7)



Low | Low Medium | High Medium | High

* $p \leq 0.05$
** $p \leq 0.005$

The profile provides an operationalization of the multi-syndrome concept of alcoholism. It is theoretically predicated upon a multi-dimensional model of alcoholism. Each dimension of the primary scales 1 to 16 is rationally and statistically independent. However, despite psychometric independence, some of the scales are correlated, particularly the second order scales A to G. Second order scales D1 and D2 correlate at 0.77. This is the case because D2 was constructed as a validity check on scale D1 as well as being a measure of deterioration associated with alcohol abuse which, in turn is operationally independent of the 16 primary scales. It should not deviate from D1 by more than one standard deviation (2 stem scores in either direction). If it does, the individual's responses have to be assumed to be invalid unless some sound explanation is to hand. All the protocols in the sample are considered valid in this respect. Primary scales 3 and 5, and second order scale B are bi-polar. The authors, Wanberg, Horn & Foster (1977) and Horn et al (1984), provide guidelines for interpreting the instrument; this is relied upon for the present interpretation.

The overall purpose of this instrument in the treatment setting is to discern variations in symptomatic behaviour with regard to drinking habits, reasons for drinking and both positive and negative outcomes from heavy drinking. This enables therapists to provide more efficient and focused treatment. Of course, more information than is provided by this instrument is required in order to make a treatment plan for any one individual. Particularly important is the

additional information needed about the social contexts of a given person.

What does the AUI profile tell us about the sample? The first point to note about the profile is its overall shape which is quite contained, having an absence of extreme scores; none of the scales are scored in the low or the high quadrants. Also, with one exception, the scores for WinCode 1 and WinCode 7 mirror each other.

The most pronounced elevation is on scale No. 2, Mental Benefit drinking; this indicates a reason for drinking, namely to enable the individual to think and to work more smoothly and to have more congenial thoughts as opposed to the indications of scale 1. Scale 1 Social Benefit drinking is scored in the low medium section indicating that alcohol is not relied upon greatly for facilitating social activity. Scale 3 is a drinking style descriptor on which the present sample is located around the middle of this bi-polar scale ranging from solo drinking to gregarious drinking. Their score is indicative of elements of both styles of drinking, that is to say, individuals would have a routine of drinking in pubs and other social venues as well as spending time drinking alone or secretly. Scale 4 describes another drinking style on which this sample tends towards continuous daily drinking but not excessively so, since brief periods off alcohol are also indicated by the scalar position. A psychological component is inherent in this scale suggesting a tendency to be pre-occupied with alcohol even when not consuming it. Scales 5,6 and 7 are descriptive of a group

that spends a lot of its time actively drinking with few periods of abstinence, is only moderately guilt-ridden about such behaviour and which does not use alcohol in a very pronounced manner to alter mood states. A picture of an epicurean motive emerges. The group likes drinking alcohol and does not worry or feel excessively guilty about such indulgence. Despite this sort of self report some have attempted to stop drinking prior to the current attempt (Scale 8). Scale 9 gives an indication of why this should be, at least for some cases. Loss of behavioural control refers essentially to things like suffering blackouts, passing out altogether, or being belligerent when drinking. The sample do not score highly on this scale, exhibiting only moderate degrees of such behaviour. Similarly, on scale 10 the sample score is quite low on Social Role Maladaptation, indicating a low incidence of events such as losing one's job, or being arrested because of heavy drinking. In the main, the impression is that the sample here have not progressed in a drinking problem to the extent that a noticeable degree of social deterioration has ensued. Scale 11 achieves a much higher elevation than scale 10, indicating that the consequences of heavy drinking are more marked insofar as psychological problems are concerned as opposed to social problems. The psychological costs concerned are states like confused thinking, frightening sensations etc., when sobering up. Scale 12 points to a more moderate degree of physical withdrawal symptoms. Perhaps this is not too surprising given the indications of scales 13 and 14. These scales show that

the use of drugs other than alcohol is not a feature of this sample and that the daily quantity of alcohol consumed is on the low side, at least in comparison with this instrument's norms. It is notable that the WinCode 7 group consume more than the rest of the sample, to a statistically significant extent. However, the Wincode 7 score is still contained within the low medium range of the instrument (scale 14). More of the sample report marital problems leading to drinking (scale 15), than see their drinking as causing marital strife in the first place (scale 16).

The most pronounced characteristics of drinking style, motivations and consequences, to emerge from a consideration of the AUI profile of primary scales are descriptive of a group of individuals who tend to conform socially but use alcohol both openly and secretly to control mental functioning. Although consumption is well in excess of normal community standards, by comparison with the norms of the test, the sample tends to consume fairly moderate quantities of alcohol. They accumulate psychological symptoms more often than they do social symptoms, and efforts are made to show a conforming and respectable face to the world. These efforts to maintain respectability entail a requirement to lead something of a double life, sneaking drinks in private.

The second order scales A to G tend to confirm the above description. The highest elevation is on scale B which indicates a pattern of fairly continuous drinking and a pre-occupation with alcohol leading to behaviours such as sneaking drinks and hiding supplies of alcohol around the house. The

items for this scale are taken from primary scales 4 and 5. Scale D2 is the next highest scoring scale among the second order scales but it is still within the mid-range of the profile. It indicates a degree of experimentation with a deteriorated style of drinking like taking low grade alcohol when none other is available, not eating during drinking binges, and drinking to relieve hangovers.

The general involvement with alcohol scale (the G scale) has already been commented upon. It is well named a General Alcoholism Scale since its items are derived from scales across the profile, with the majority coming from primary scales 1,4,5,9,10,11,12, and 14. The G scale is in the low-medium range as noted earlier.

A comparison of WinCode 1 and WinCode 7 on this profile indicates little divergence between the two groups on the primary scales; only daily quantity of alcohol consumed differentiates the two groups with WinCode 7s being the heaviest consumers ($P < 0.05$).

On the second order scales, statistically significant differences appear between WinCode 1 and 7 on scale A ($P < 0.05$), with WinCode 7 having the lower mean score on Self-Enhancing drinking. Scales B and C are not statistically significant, but scales D1 and G are. Scale D1 achieves a level of significance of $P < 0.004$, with WinCode 7 experiencing greater deterioration. WinCode 7s also have a higher general alcoholism score ($P < 0.05$, one-tailed).

It is perhaps not surprising that more of the second order, or "broad alcohol use dimensions" reach statistical

significance because they entail a wider pool of possible variations. That is to say, the primary scales are psychometrically and theoretically independent; however, they are also correlated to varying degrees. The second order scales attempt to tap the broad areas of influence that are superordinate to the unitary independent dimensions (re-correlated). Factor analysis of the primary scales resulted in the 6 broad, second order factors. Primary scale items were used to compile the second order scales in preference to primary scale scores to ensure that overlap between the second order scales did not occur creating dependencies among these scales.

In conclusion, there is some justification for arguing that the completers of WinCode7 had a somewhat more severe alcohol problem than the rest of the sample; but the difference is not impressive.

SEVERITY OF ALCOHOL DEPENDENCE QUESTIONNAIRE (SADQ)

SADQ mean 28.8 : S.D. 12.77 : Range 2 - 57

The SADQ score with its mean of 28.8 has already been noted and commented upon above. This measure has achieved widespread acceptability, as a quick measure of alcohol dependence over the last few years. It is grounded in the notion of the Alcohol Dependence Syndrome as described by Edwards & Gross (1976). The Alcohol Dependence Syndrome is conceived of as a core cluster of symptoms which are essentially clinically derived, and which are differentiated from other features of an alcohol problem, termed "Alcohol Related Disabilities". These disabilities are related to or

consequent upon excessive drinking. The syndrome construes dependence in terms of a continuum ranging from weak to strong. It is provisionally defined by seven features representing varying degrees of severity. These are :-

1. Narrowing of drinking repertoire.
2. Salience of drink-seeking behaviour.
3. Increased tolerance to alcohol.
4. Repeated withdrawal symptoms.
5. Relief, or avoidance of withdrawal by further drinking.
6. Subjective awareness of the compulsion to drink.
7. Re-instatement of the syndrome after abstinence.

SADQ is a measure of the degree to which individuals experience the syndrome of Alcohol Dependence. It ignores the more diverse aspects of an alcohol problem that would be discussed as Alcohol Related Disabilities (Edwards et.al. 1976).

The notion of a central unifying concept of alcohol dependence is not without an element of controversy in theoretical debate, with the old disease notion entering the arena again through the back door. The concept of a severity of dependence now has almost universal clinical appeal. It was as a clinical concept that Edwards and his colleagues on a World Health Organization working party formulated it in 1976. SADQ does not attempt to evaluate the 7 features of the syndrome listed above. Rather it focuses upon the experience of withdrawal symptoms, the rationale being that these would be amenable to measurement in a fairly straightforward fashion, whereas other features of the syndrome might not be

easily discerned, since they are based upon complex individual and clinical judgements.

The current, 20 item self-completion questionnaire asks respondents to answer the questions with a recent typical month of heavy drinking in mind. The cut-off point for severe dependence is 30, and 29 or less for moderate to mild dependence. The authors provide evidence of high test-retest reliability, construct and concurrent validity (Stockwell et.al.1985). Although it was not designed to assess all seven parameters of the alcohol dependence syndrome, it does correlate positively with the first, listed above as "Narrowing of drinking repertoire".

SADQ is used in this study to locate the sample on the proposed continuum of alcohol dependence such that it can be compared with other treatment samples reported in the literature. It measures an important clinical concept and has relevance for treatment planning. What it does not have is predictive validity in the medium and long term. Indeed, if it had, this thesis with its focus on Affect Balance would be stillborn. The sample is poised with a mean just below the cut-off point for severe dependence, perhaps low by the authors' norms, and certainly when taken in conjunction with the AUI G-score, yet higher than two relevant local samples of recent date.

Table No. 12

DRINKING RELATED LOCUS OF CONTROL (DRIE)

WinCode all		WinCode 1-6		WinCode 7	
\bar{X}	S.D.	\bar{X}	S.D.	\bar{X}	S.D.
8.38	5.76	9.01	5.58	8.52	6.08
-----		-----		-----	
N = 166		N = 105		N = 61	
-----		-----		-----	

It is noted that WinCode 7 cannot be distinguished from the rest of the sample on this measure of locus of control of drinking behaviour. The scores tend to be somewhat higher in the external direction than most of the published research using this measure with treatment samples, where a mean score of 6 and a standard deviation of 4 tend to be reported in a predominantly American literature (Donovan & O'Leary 1978; and Walker et.al 1980).

A high external locus of drinking control tends to be supportive of the image of the sample that emerged from a consideration of the AUI scales above. It is a negative aspect of an individual's drinking problem to be overturned by treatment. An internal locus of control is indicative of a sense of personal power. The specific focus of the drinking-related internal-external locus of control on drinking behaviour gives a measure of the extent to which the sample construe drinking alcohol and its consequences as independent factors external to themselves, such as the company they are in, fate etc.

It is a function of treatment to enhance an individual's sense of mastery over drinking, and to encourage the assumption of responsibility for his own behaviour so

that he either stops drinking altogether or brings it under control.

SUMMARY OF SAMPLE DRINKING STATUS

The sample tends not to be too severely physically dependent upon alcohol. It does exhibit a high degree of external locus of control in respect of drinking behaviour. The primary reason for excessive drinking appears to be an attempt to improve mental functioning and only moderate degrees of social deterioration seem to have resulted from heavy drinking. The sample is characterised by moderately high obsessive drinking styles, but overall, only moderate quantities of alcohol are consumed, at least in comparison with a hospitalized American comparison group.

It is generally not possible to discern two separate profiles for Wincodes 7 and the rest, at the WinCode 1 stage, although some significant differences do appear in the AUI scales, the most notable being that those who do complete the study period, those individuals in WinCode 7, drink more and tend to have a moderately more severe alcohol problem generally.

Table No. 13

PSYCHOLOGICAL VARIABLES:THE CROWN CRISP EXPERIENTIAL INDEX

	W.C. all		W.C. 1-6		W.C. 7	
	\bar{X}	S.D.	\bar{X}	S.D.	\bar{X}	S.D.
Free floating anxiety	9.11	3.66	9.43	3.60	8.57	3.73
Phobic anxy.	5.26	2.89	5.25	2.78	5.28	3.10
Obsession'ty.	7.93	3.22	8.08	3.15	7.67	3.37
Somatic conc.	6.92	4.15	7.33	4.24	6.21	3.94 *
Depression	8.17	3.18	8.29	3.26	7.97	3.05
Hysteria	6.28	3.45	6.53	3.54	5.84	3.28

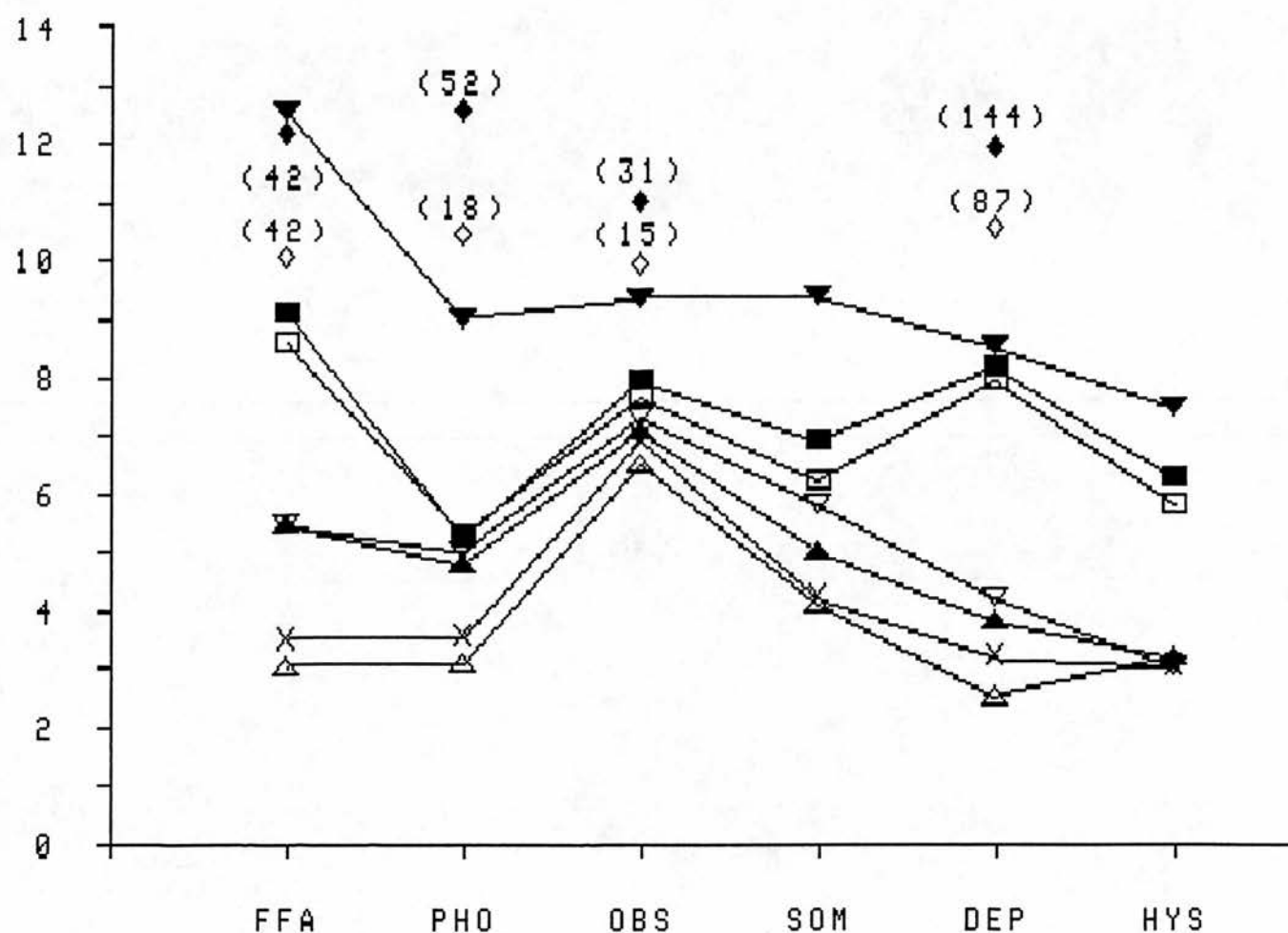
 * T-test P = <0.047 (one-tailed DF = 164)

These scales exhibit high elevations against general population norms on FFA, Som, Dep and Hys scales. The somatic concern scale achieves statistical significance on a one-tailed test at just below the 0.05 level. It is noted that the scales for both groups WinCode 1-6 and WinCode 7 shadow each other with WinCode 7 having the lowest scores. (See the diagram on Page 285 for a graphic illustration). It has already been observed that the completers of WinCode 7 have higher consumption rates and general alcoholism scores at the beginning phase, than the rest of the sample. It is therefore surprising that they should also produce lower somatic concern scores. The expectation was that they would score higher than the rest on this scale by reason of their heavier drinking and its consequent effect upon physical well-being. No obvious explanation of this finding is to hand.

The instrument is versatile and it has a large and varied range of standardization samples through which test norms were established.

CROWN CRISP EXPERIENTIAL INDEX PROFILE

(For Wincode 1-6 & Wincode 7)



Legend

- | | |
|--|--|
| △ 1. Suburban Males | □ Wincode Seven |
| X 2. Rural Males | ■ Wincode One-Six |
| ▲ 3. Rural Females | ◇ Specific Clinical Groups (Male) (n) = Number's |
| ▽ 4. Suburban Females | ◆ Specific Clinical Groups (Female) (n) = Number's |
| ▼ 5. Inpatients with psychoneurotic illness (both sexes) | |

EXPLANATION OF NORMATIVE DATA ON DIAGRAM

The 4 lines plotted on the bottom half of the diagram represent mean sub-scale scores for the normal population for suburban males and females (the 2 bottom lines); and rural males and females (next 2 lines). Females are represented by a dotted line; males by a broken line with dots between the dashes.

Next, the sample scores are plotted. The WinCode 7's are drawn with crosses at their ordinates and the rest, WinCodes 1- 6, are denoted by having dots at their ordinates. The broken and dotted lines above the sample scores represent mean scores for in-patients suffering from psycho-neurotic illness. The squares and circles above and below the trend line for psycho-neurotic in-patient scores represent mean scores of clinical diagnostic groups specific to the 4 individual sub-scales: circles represent females, and squares, males.

This instrument has been used with a wide variety of populations and is capable of producing valid change scores. It has acknowledged validity and reliability.

It should be noted from the diagram that the 2 WinCode lines lie subjacent one to the other except where they diverge above the Somatic concern scale. Although these scores fall a good deal short of the various clinical groupings, they are also well elevated on four scales, above the general population scores. The sample as a whole appear to be quite anxious and depressed. The hysteria scale is also elevated: this "Hys." scale is the least valid of all the

scales because of the theoretical difficulty that surrounds the concept of hysteria. It is possible that this scale is in fact a measure of extroversion. The Som. scale elevation for WinCodes 1 - 6 has already been mentioned. The FFA and Dep scales have importance for clinical monitoring.

Table No. 14

SELF ESTEEM

WinCode all		WinCode 1 - 6		WinCode 7	
\bar{X}	S.D.	\bar{X}	S.D.	\bar{X}	S.D.
52.66	22.17	52.25	21.81	53.38	22.93

No significant differences appear between these groups on self-esteem. All score in the middle range of the scale, indicating low self-esteem, much as one would expect given the high scores found for anxiety and depression that were observed in the Crown-Crisp Inventory, and indeed from a reading of the alcoholism literature. With treatment there would be a strong expectation that the self-esteem score would increase. No significant differences exist in the sample on self-esteem between the WinCode 7 group and the rest, at the beginning phase.

SUMMARY OF PSYCHOLOGICAL VARIABLES

The sample display a degree of psychological impairment evident from the markedly high levels of anxiety, depression and somatic concern as measured by the Crown-Crisp inventory, and the low levels of self-esteem. It was noted earlier that the sample was psychologically sensitive in the sense that withdrawal problems tended to be manifest in this area. Those who completed the study period

did not exhibit any particular psychological robustness, at least none was detected in the WinCode 1 stage.

SOCIAL VARIABLES

Table No. 15

The Modified Social Behaviour Assessment Schedule, (S.B.A.S.)
Disturbed Behaviour and Burden Scales

	W.C. all			W.C. 1-6			W.C. 7		
	\bar{X}	SD	Range	\bar{X}	SD	Range	\bar{X}	SD	Range
*D.B.	10.8	7	0-30	11.0	7	0-30	10.3	7	0-28
£B.E.	12.9	8.4	0-37	13.1	8.3	0-37	12.5	8.5	0-37
N = 166			N = 105			N = 61			
* = "Disturbed Behaviour"				£ = "Burden Experienced"					

What is measured here is an objective rating of disturbed behaviours present (21 such behaviours listed), and a subjective rating of burden felt in experiencing these behaviours. Both groups produce a result indicative of multiple problems with a concomitant degree of burden endured as a consequence. The scores are well below the maximum possible. The 21 items comprising this scale are scored 0-2, therefore the range in each case is 0 - 42. However, given the serious natures of the behaviours listed, it is highly unlikely that a sample such as the present one would have scored in the higher reaches of the range available.

Table No. 16

SOCIAL BEHAVIOUR ASSESSMENT SCHEDULESocial Support-Scales

W.C. all			W.C. 1-6			W.C. 7		
\bar{X}	SD	Range	\bar{X}	SD	Range	\bar{X}	SD	Range
*E.S. 3.27	2.37	0-14	3.28	2.36	0-14	3.26	2.41	0-14
EP.S. 2.04	1.92	0-11	1.98	1.88	0-11	2.13	2.00	0-10
N = 166			N = 105			N = 61		

* E.S. = "Emotional support" EP.S. = "Practical support"

It is evident that on this measure little emotional or practical support was available to the members of the sample. Again there are no differences between the groups. Both sorts of support, mental and physical, are known to be important for the well-being of individuals. It is not possible to provide relevant comparison sample means to help evaluate these scores, but as a percentage of the total possible scores they represent only 32% and 25% respectively. These are very low proportions indeed. The conclusion must be that the sample received only minimal, emotional and practical support.

Table No. 17

INTERVIEW SCHEDULE FOR SOCIAL INTERACTION (I.S.S.I.)

	W.C. all		W.C. 1-6		W.C. 7	
	\bar{X}	S.D.	\bar{X}	S.D.	\bar{X}	S.D.
AVAT	4.73	2.21	4.44	2.27	5.23	2.04*
ADAT%	53.26	28.5	49.33	27.71	60.02	28.80*
AVSI	7.24	3.76	7.27	4.01	7.20	3.34
ADSI	9.19	4.16	8.84	4.32	9.80	3.83

	N = 166		N = 105		N = 61	

* = P < 0.05 (T-test two-tailed)						

What this table shows is that the sample as a whole suffers from a high degree of impoverishment in their primary relationships, at least in comparison with the instrument's Australian norms. The normative scores are available for a variety of populations. The mean scores of the present sample are a good deal lower than the lowest scoring normative general population group which is the divorced/separated category.

The scores for availability of attachments (AVAT), and adequacy of attachments (ADAT%), are very low both for WinCode 7 and for the rest of the sample. WinCode 7 score significantly higher than the rest on these two scales. This is perhaps not surprising when one recalls that nearly 70% of WinCode 7 are married. The adequacy-rating is perhaps less to be expected. It is the case then, that the group of completers have more attachment relationships available to them, and they also tend to be more satisfied with the quality

of these relationships than the rest of the sample are with their relationships. It has been reported by the authors of this instrument (Henderson, Byrne and Duncan-Jones 1981), that personality variables are highly likely to influence the subjective rating of adequacy scales based upon correlations observed in a large-scale Australian community survey.

The Interview Schedule for Social Interaction is predicated on Bowlby's theory of attachment (Bowlby 1969). The attachments measured and the respondents' judgement of the adequacy of such relationships, are the close-bonded relationships of Bowlby's theory. All other relationships are subsumed under the rubric of social integration. Overall, the sample score very poorly in both of these areas. In addition to Bowlby's work on the social bond hypothesis, the authors of the I.S.S.I. draw upon the work of the Boston Sociologist Robert Weiss (1969) with particular reference to the distinction he makes between relationships and provisions of social relationships. Adequacy ratings refer to an evaluation of the "provisions" obtained from the two sorts of relationships Weiss measured. This gives rise to a fourfold classification :-

1. Those with high availability and adequacy.
2. High availability but low adequacy (needs not being met by available relationships).
3. Low availability but high adequacy (indicative of a high degree of self-sufficiency in social isolation).
4. Low availability and low adequacy (here, individuals do not have the social resources available to meet their needs.)

The majority of the population (at least in Australia!) appear to reside in the favourable quadrant with high availability and adequacy of social relationships. The present study sample are much less favoured.

AFFECT BALANCE

Problems arose in the data analysis because of the scoring procedure used with the hand-scored delighted-terrible scale (a 7 point scale), where + or - 3 rotated around a mid-point valued at zero. With a hand scoring procedure used to obtain individual profiles for clinical purposes, the problem of having the mid-point of the scale 4, equal to zero, and counting as positive values 5 to 7, and negative, values from 1 to 3 does not arise. It was not possible to treat the mid-point as zero in the present data analysis, and account for the number of domains endorsed at the same time because the mid-point value zero acted exactly the same as a domain non-selection.

3 options were available to deal with this problem :-

1. To ignore it altogether.
2. Assign to it the lowest positive value of +1
3. Assign to it the lowest negative value of -1

In the data analysis it was decided to assign to it the lowest positive value, partly because the label attached to it on the delighted-terrible scale, "mixed feelings", suggested that that ought be done. However, computations were carried out treating the zero in each of the three modes described above, and the overall result was that it hardly influenced the results at all whichever solution had been

chosen.

The four scalar options were :-

1. Negative totals coded Negtot = -1 to -3; 4 being omitted.
2. Positive " " " " Postot = +1 to +3; 4 being omitted.
3. Positive totals +4 as +1, coded Postot4 = +1 to +4; 4 acts as +1
4. Negative totals +4 as -1, coded Negtot4 = -1 to +4; 4 acts as -1

Three separate summary scores for positive and negative scores across the 13 domains were subtracted to create a balance-scale, the 3 available options being :-

5. Net totals coded netot = postot - negtot (4 is out)
6. Nettot4 coded netot4 = postot4 - negtot (4 = +1)
7. Net totals 5 coded netot5 = postot - negtot4 (4 = -1)

The scales used in this data analysis are:

(3) postot4 (1) negtot balance score = (6) Postot4 - negtot

Mean scores were computed for each of the possible 13 domains endorsed as well as standard deviation, standard error of the mean, and variance. The totals were then treated in the way described above to obtain an Affect Balance Score. The table following lists the 13 domains with means and standard deviations.

Table No. 18

AFFECT BALANCE

	W.C. All		W.C. 1-6		W.C. 7	
Domains	\bar{X}	SD	\bar{X}	SD	\bar{X}	SD
1. Family life	3.95	1.97	3.64	2.02	4.48	1.78
2. Marriage	2.84	2.59	2.39	2.47	3.61	2.65
3. Neigh.Hood.	4.55	1.66	4.30	1.83	4.97	1.21
4. Parents	3.09	2.52	3.03	2.40	3.21	2.74
5. Work/unemp.	3.73	1.74	3.76	1.66	3.67	1.88
6. Financial	3.92	1.42	3.87	1.41	4.00	1.43
7. Health	4.61	1.56	4.43	1.58	4.92	1.49
8. Friendships	4.45	1.47	4.25	1.49	4.80	1.38
9. Housing	4.39	1.88	4.32	1.84	4.51	1.95
10. Religion	3.17	2.29	3.23	2.25	3.07	2.37
11. Leisure	4.22	1.52	4.10	1.56	4.41	1.48
12. Organization	2.56	2.59	2.44	2.58	2.79	2.63
13. Legal sitn.	1.98	2.36	1.79	2.25	2.33	2.54
	N = 166		N = 105		N = 61	

Positive and negative totals

WinCode all			WinCode 1-6			WinCode 7		
postot4 - negtot			postot4 - negtot			postot4 - negtot		
\bar{X}	11.44	4.36	\bar{X}	10.54	4.83	\bar{X}	12.98	3.56
S.D.	5.24	4.59	S.D.	5.08	4.85	S.D.	5.18	4.03
= netot4			= netot4			= netot4		
\bar{X}	7.07	8.98	\bar{X}	5.71	9.02	\bar{X}	9.43	8.48
S.D.			S.D.			S.D.		

T-test P < 0.010 164 DF two-tailed

The table shows a statistically significant difference in the Affect Balance score (netot4) between WinCode 7 and the rest of the sample at this beginning phase. However, despite the WinCode 7 superiority on this measure, it must be set

against the possible range of the instrument which is 13×7 , i.e. 13 to 91. To score 13, each domain would require to be endorsed at scale point 1 (terrible), and to score 91, each domain would have to be scored 7 (delighted). Of course, not every respondent would endorse each domain; most however would endorse around 10 domains. Set against these limits, a WinCode 7 score of 9.43 is low, and the rest of the sample with a score of 5.71 is particularly low. It is noteworthy that none of the domain score means are located in the positive range of the scale (5 to 7).

The emergent picture overall is of a sample that is substantially dissatisfied with life as a whole. The fact that the 3.72 statistically significant difference between the sub-group of completers and the rest of the sample is made up of small incremental gains across the 13 domains has importance for a theory of Affect Balance, in that it is the overall balance score that is thought to be crucial, regardless of how it is arrived at.

SUMMARY OF SOCIAL VARIABLES

The general picture to emerge from a scrutiny of the variables under consideration in this section, is of a sample burdened with personal difficulties and with low levels both of emotional and practical support; and reporting low levels of satisfaction with life as a whole.

GENERAL SUMMARY

The demographic characteristics have been described. The majority of the sample population are married men in social class five who are still in employment. Very few women

are represented in the sample. The drinking profile tends to indicate mild to moderate degrees of alcohol dependence and problems consequent upon heavy drinking. The social profile is one of multiple dissatisfaction with life.

The next chapter looks at WinCode 7 in detail across the study period. It is evident from the data presented in this chapter that a separate, rounded profile of WinCode 7 at the WinCode 1 stage cannot be derived. This supports the view that extraneous variables might have had an important influence on outcome.

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CHAPTER 5

DATA ANALYSIS II

In the previous chapter the sample was described and, apart from minor differences, it did not prove possible to derive a rounded profile of completers that distinguished them from the rest of the sample in a significant way, at the beginning of the study on the variables included. This chapter will first of all address the before and after aspect of the design by looking at outcome and by presenting data on the 61 cases that comprise WinCode 7. That is a necessary condition since these were the only cases to complete the study period. Thereafter, process measures will be looked at and the results of a correlation analysis will be presented followed by an analysis of variance to explore the impact of Affect Balance on process measures.

DRINKING VARIABLES

The Alcohol Use Inventory (AUI) presents inevitable difficulties when it is presented to individuals who have undergone an extensive period of treatment for their drinking problems in an abstinence oriented programme. Answering questions about drinking behaviour and attitudes does not lie easily with such individuals who, by virtue of their continuance in the programme, have either been abstinent, or virtually abstinent. Problems arise primarily in two ways with the collection of valid data on drinking variables with such a population. Most obviously respondents have difficulty in making clear responses to questions that no longer appear to be too relevant to them. This may not be too devastating a

problem for a piece of research if ambiguity is spread evenly across the sample, and a core of valid responses can be obtained.

A more insidious and widespread problem is the issue of under-reporting drinking lapses and generally faking good. The type of programme studied in this thesis appears to be particularly prone to this sort of problem. All of the subjects studied had been exposed to a strong abstinence ideology transmitted by voluntary clinic staff who, in the main, have been programme "graduates" themselves and tended therefore, towards the craftsman approach mentioned in the first chapter. The attitudes they exhibit tend to inhibit disclosure of lapses by clients during the course of the treatment programme such that data on drinking need to be treated carefully. I am aware from first hand experience that the self-reports of drinking during the study period of this thesis represent a gross underestimate of the actual amount of drinking that occurred. The table below is of interest in this respect and provides further justification for using WinCode 7 data. It is evident from a scrutiny of Table 19 that those individuals who reported having a drink during the treatment phase are grossly over-represented in the WinCode 7 group. Only 61 instances of drinking were reported throughout the study period by the total sample, and of these, 69% were reported by persons in the WinCode 7 category.

ANALYSIS OF REPORTS OF DRINKING

PHASES

Wincode	No. Cases	1	2	3	4	5	6	7	
1 %	41	0							0
2 %	20	0	6 40						6
3 %	22	0	1 6.6	2 20					3
4 %	10	0	1 6.6	2 20	1 5.9				4
5 %	7	0	1 6.6	1 10	2 1.7	1 10			5
6 %	5	0	0	0	0	0	1 14.3		1
7 %	61	0	6 40	5 50	14 82.3	9 90	6 85.7	2 100	42

N = 166 0 15 10 17 10 7 2

N = 125 106 105 83 73 66 61

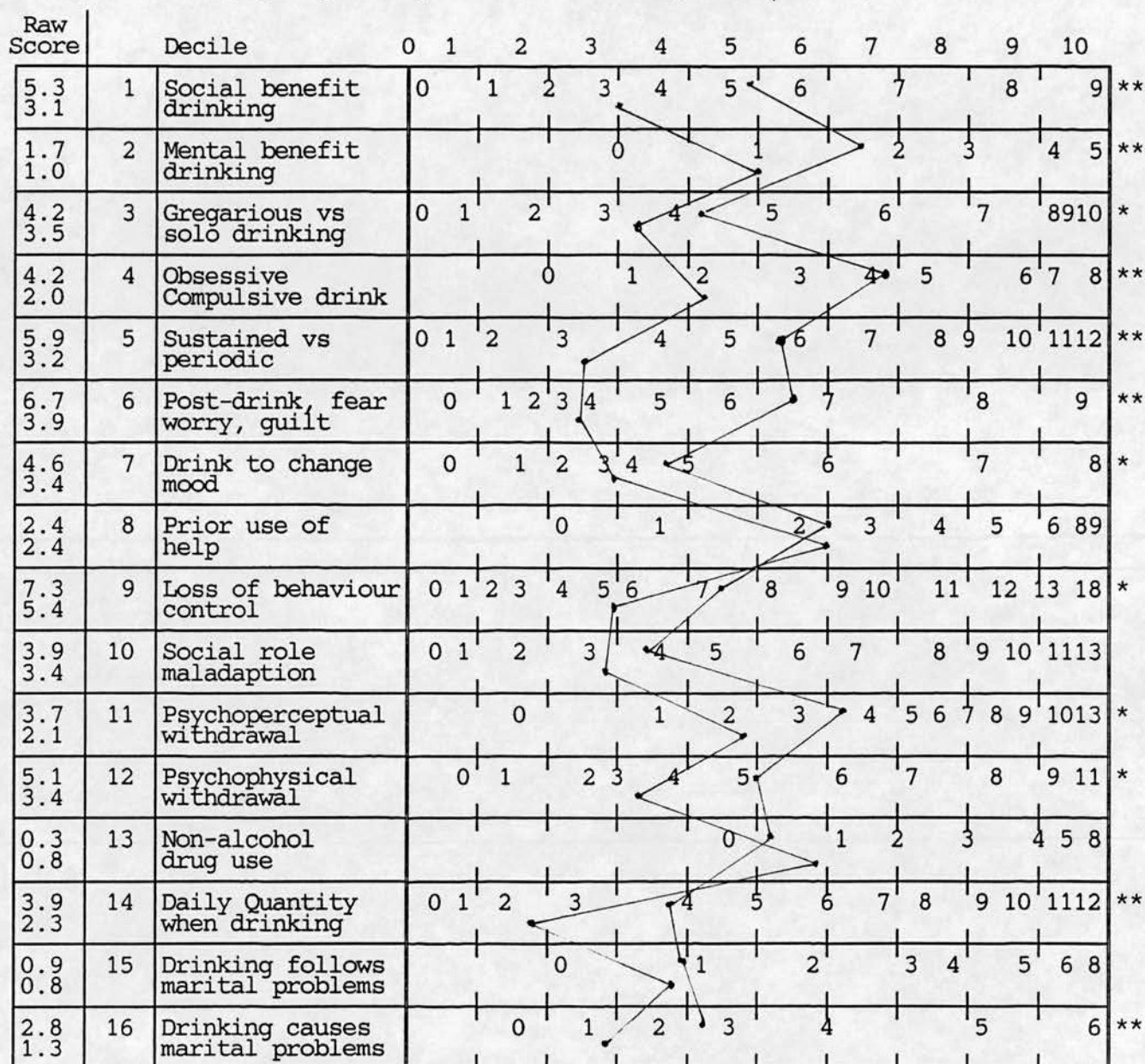
It is clear from the table that, with the exception of the 20 persons in WinCode 2 who accounted for 40% of the drinking reported at phase 2, WinCode 7s were responsible for the great majority of such reporting. Because of the decreasing numbers across the table, the percentages have a tendency to exaggerate somewhat. Nevertheless, one would expect those who are about to drop out to be more prone to drinking. The commonsense explanation of these figures would appear to be that the successful outcome group, WinCode 7, were more committed and tended to take their work in the programme more seriously than the rest, consequently their self-reports of drinking were evidence of this. An alternative explanation might be that because they had more serious drinking problems at the outset, they were more prone to lapse during treatment. However, it should be remembered that the difference exhibited in daily intake by WinCode 7, was greater than the rest of the sample to a statistically significant degree, but the quantities consumed by all WinCodes were not large by comparison with normative data provided for the Alcohol Use Inventory.

THE ALCOHOL USE INVENTORY

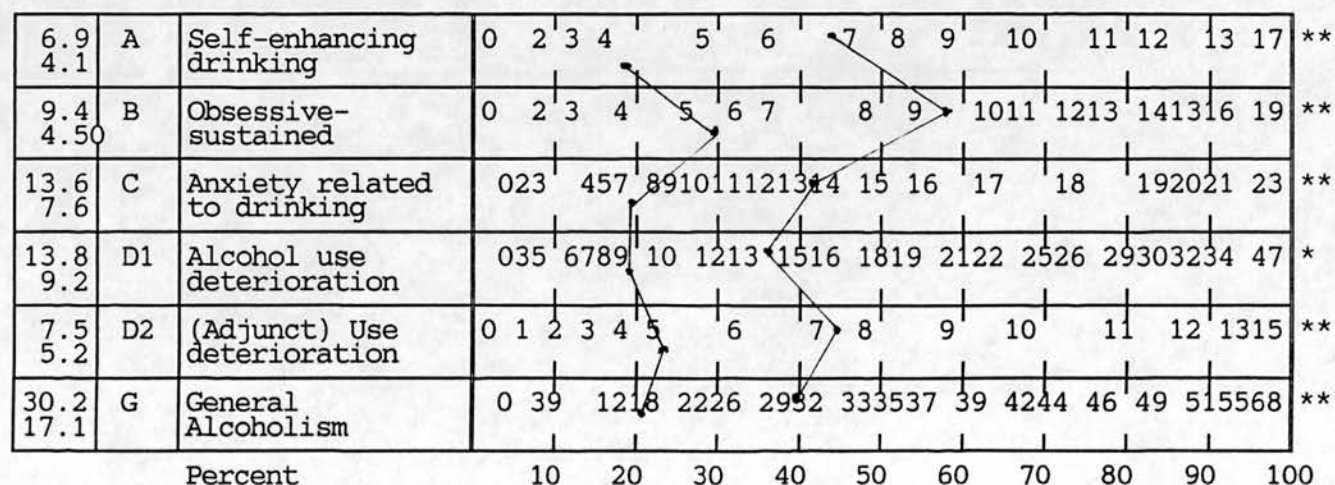
All the scales of the AUI except Nos 8 & 13 which one would expect to be invariable, and No 10, which was low initially, show highly significant statistical shifts in the expected directions. All the second order scales show highly significant changes. These are results which one would anticipate given the fact that the sample are the completers in an alcohol treatment programme (see profile overleaf). That

ALCOHOL USE INVENTORY PROFILE

For Wincode 7 at phase 1 and at phase 7
(Top figure = phase 1 Lower figure = phase 7)



PRIMARY SCALES



GENERAL SCALES

Percent

Low | Low Medium | High Medium | High

* p < 0.005
** p < 0.001

is, one would expect significant changes in the positive direction, but it is less clear how low one might expect the scores to go. There are no appropriate general population norms to compare the sample with. The major point of interest for discussion concerning these major reductions in AUI scale scores, is that these improvements can be seen as a function of the recovery period, since it was not possible to distinguish WinCode 7 from the rest of the sample except in minor ways.

Table No. 20

SEVERITY OF DEPENDENCE (SADQ)

Phase 1		Phase 7	
\bar{X}	S.D.	\bar{X}	S.D.
28.59	13.60	29.37	15.69

No significant change is observed here for severity of dependence between the beginning and the end of the study period. This is exactly as one would expect given the instrument's instruction to consider a typical drinking period. The closeness of the figures attest to the instrument's reliability. If the slightly increased score at the end of the study period (0.78) means anything at all, perhaps it taps a tendency for recovering problem drinkers to look back in horror at their drinking, and emphasise the withdrawal problems that they experienced.

Table No. 21

DRINK RELATED LOCUS OF CONTROL (DRIE)

Phase 1		Phase 7	
\bar{X}	S.D.	\bar{X}	S.D.
8.52	6.08	1.50	3.10 *
N = 61		* P < 0.01 60 DF	

Drinking related locus of control is perhaps a more succinct measure of change at the end of the study period because it is not unduly dependent upon drinking behaviour, but rather taps attitudes to such behaviour bearing in mind the caveat about the influence of the programme ideology on selective responding. On the evidence of these figures, it is clear that the programme successes have effected substantial change on this measure, from an outer to an inner orientation. They clearly see themselves as more powerful and personally responsible in drinking situations compared with the opposite state of affairs at the beginning of the study period. Wincode 7 are comparable with the general population scores for DRIE.

SUMMARY OF DRINKING VARIABLES

On the basis of the evidence relating to drinking variables for WinCode 7 at the end of the study period we can note that this group of completers comprise 36.74% of the sample: just fractionally over one third of the total sample. Here we have another exemplar of the "rule of one third" noted elsewhere.

With regard to drinking variables, treatment appears to have been effective in operating to reduce the drinking problem on the multiple dimensions measured by the AUI and the

DRIE scales. In interpreting these results, one has to be mindful of the caveats mentioned previously about the difficulties encountered by respondents in answering questions about drinking behaviour when they have been exposed to a strongly abstinence oriented treatment programme. It was noted that the WinCode 7 tended by far to be the most forthcoming in reporting drinking lapses, when commonsense and personal familiarity with the programme suggest that they were probably the group that lapsed least. Overall then, the results of the analysis of drinking variables were much as one might predict them to be.

Table No. 22

PSYCHOLOGICAL VARIABLES

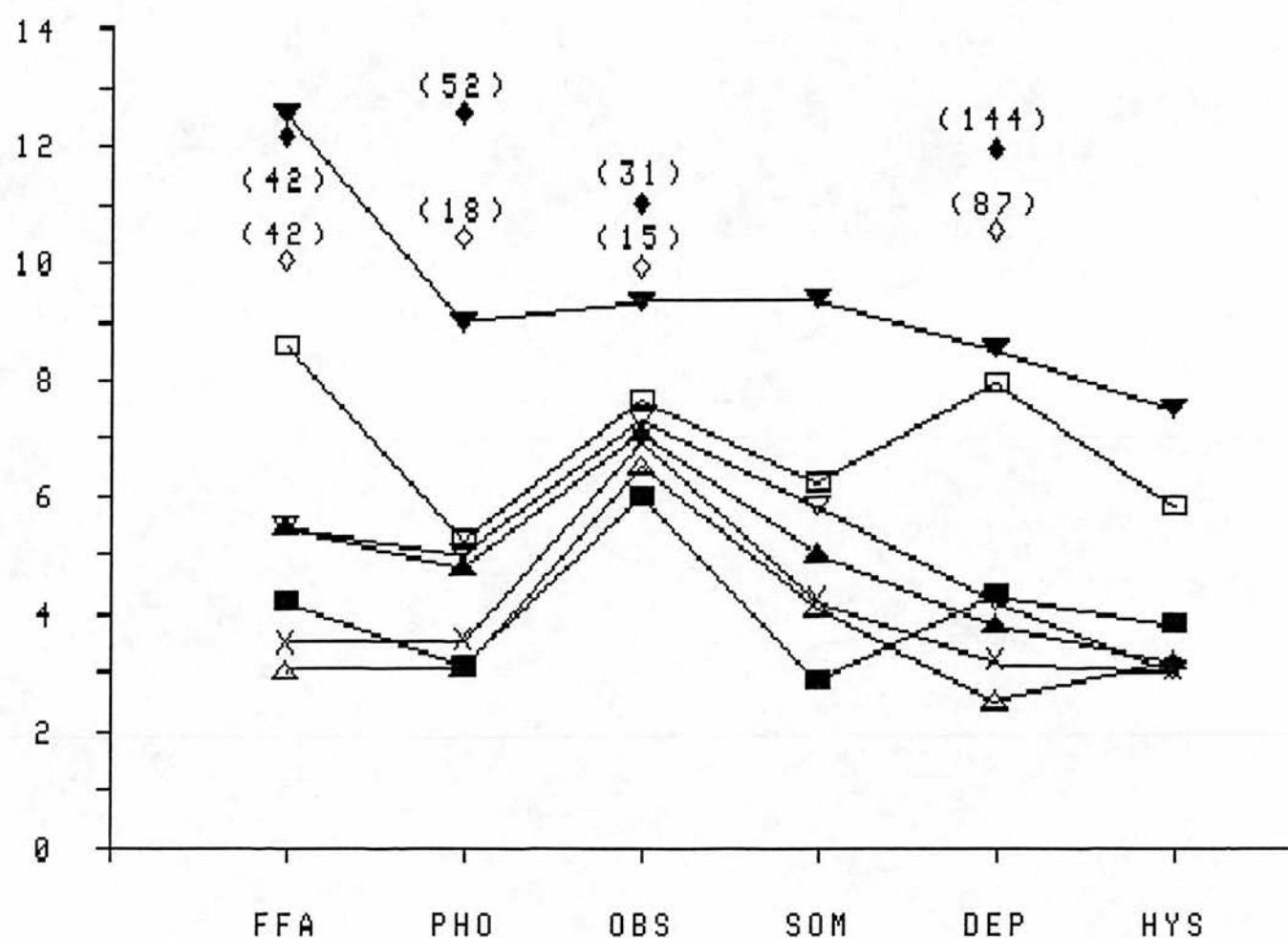
The Crown Crisp Experiential Index for WinCode 7.

	Phase 1		Phase 7.		
	\bar{X}	SD	\bar{X}	SD	
FFA	8.57	3.70	4.21	3.71	**
Pho	5.27	3.10	3.14	2.59	**
Obs	7.67	3.37	6.02	2.94	*
Som	6.21	3.94	2.85	2.71	*
Dep	7.96	3.04	4.34	3.12	**
Hys	5.83	3.27	3.81	3.22	**

** P<0.001 60 DF			* P<0.005 60 DF		

CROWN CRISP EXPERIENTIAL INDEX PROFILE

(For Wincode 7 at Phase One & Seven)



Legend

- | | |
|--|--|
| △ 1. Suburban Males | □ Wincode Seven, Phase One |
| X 2. Rural Males | ■ Wincode Seven, Phase Seven |
| ▲ 3. Rural Females | ◇ Specific Clinical Groups (Male) (n) = Number's |
| ▽ 4. Suburban Females | ◆ Specific Clinical Groups (Female) (n) = Number's |
| ▼ 5. Inpatients with psychoneurotic illness (both sexes) | |

An examination of these scores as plotted on the graph shows that none of the WinCode 7 scores at the final phase are elevated above the test norms for the general population. There is a marked improvement from the phase one scores of these 61 completers, particularly on the A,D and H scales. The slight drop below the general population line for scale S on the graph probably requires no explanation given the SD of 2.71 for this scale. Alternatively one might perceive this low S score after six months abstinence or near abstinence, as a slight halo effect caused by recovering problem drinkers tending to emphasise the worst features of their previous state, and in consequence improving their present state by comparison.

The WinCode 7s present, on the evidence of these measures, a picture of sound mental health. All the changes between phases 1 and 7 are statistically significant.

Table No. 23

SELF ESTEEM

Phase 1.		Phase 7.	
\bar{X}	SD.	\bar{X}	SD.
53.37	22.93	78.19	23.92 *

* $P = < .001$ DF 60

A sizeable shift of 24.82 percentage points makes this change statistically highly significant. Perhaps one might expect this result given the very low score at phase one. The phase 7 score is well within a general population range. The importance of favourable attitudes towards self is regarded as being of crucial importance by all personality theorists, and

therapists of all persuasions. Self-esteem has long been believed to be an important co-variant of a whole host of desirable attributes. In her influential book on concepts of positive mental health, Jahoda (Jahoda 1958) gave considerable weight to "attitudes of an individual towards his own self". Coopersmith, the author of this Self-Esteem measure, suggested elsewhere that there are four primary sources of self-esteem (Coopersmith 1967):

- A. A feeling of significance based upon the response of others to oneself. i.e. acceptance, attention and affection.
- B. An idea of personal competence predicated upon past achievements.
- C. A notion of power in personal relationships.
- D. A sense of virtue derived from living according to some accepted moral standard.

Coopersmith links self-esteem to "social independence and creative expression".

Rosenberg (1962,1965) has demonstrated the relationship between self-esteem and a variety of important mental health issues including anxiety, depression and social isolation. Indeed the correlates of self-esteem look like a lexicon of social and mental health terms. Linear relationships have also been demonstrated between self-esteem and the personal and social adjustment of children (Dorr, Rummer and Green 1976).

SUMMARY OF PSYCHOLOGICAL VARIABLES

The shifts in the variables accounted for in this section are all in the desired direction. They are

statistically large, and bring the sample completers back to a baseline of general population norms. Again it is important to remember that the 61 WinCode 7's were hardly distinguishable from the total sample at phase one, therefore these changes have to be accounted for by their experiences over the treatment phases. By the end of the study period these 61 individuals do not exhibit any problem-scores on any of the sub-scales of the Crown-Crisp inventory, hence their psychological adjustment in terms of this measure can be said to be normal or good. Additionally, they have increased their self-esteem rating to a significant degree such that they again fall within general population norms. We have noted that the self-esteem score is a correlate of many wide-ranging mental health concepts including most of those measured by the Crown-Crisp inventory. In summary then, this group of programme completers achieve full marks for their psychological well-being.

SOCIAL VARIABLES

Table No. 24

THE MODIFIED SOCIAL BEHAVIOUR ASSESSMENT SCHEDULE (S.B.A.S.)

Phase 1.				Phase 7.		
Wincode						
W.C. 7	\bar{X}	SD	Range	\bar{X}	SD	Range
*D.B.	10.31	7.04	0 - 28	3.90	6.00	0 - 25 **
£B.E.	12.51	8.49	0 - 37	4.79	7.12	0 - 30 **

*D.B. = "Disturbed Behaviour" ; £B.E. = "Burden Experienced"

** = P = <0.001, DF 60

One can observe an extensive reduction in the reporting of disturbed behaviour and the experienced burden or personal cost of these behaviours to the individuals concerned, from the above table. It is not possible to compare the phase 7 rating with a general population sample. However, when one considers the 21 items that comprise the list of disturbed behaviours, bearing in mind that the possible range of scores is 0 - 42, it seems quite reasonable to imagine that items like bodily aches and pains, rudeness, worry, slowness, irritability, under or over-activity etc, might well be endorsed by a general population sample which excluded psychiatric patients, and achieve a range of scores which might well be comparable with the above. The reduction of the means on this measure is indicative of significant improvement in the sample over a list of behaviours previously reported as burdensome; a 40% and 59% reduction respectively.

Table No. 25

SOCIAL SUPPORT (Mod SBAS)

Phase 1.				Phase 7.		
	\bar{X}	SD	Range	\bar{X}	SD	Range
*E.S.	3.26	2.41	0 - 14	2.78	1.95	0 - 10
**P.S.	2.13	2.00	0 - 10	1.72	1.62	0 - 7
*E.S. = "Emotional Support" ; **P.S. = "Practical Support"						

There has been no significant change in the degree of emotional and practical support received by the programme completers, indeed the means at phase 7 are slightly lower than they were at the beginning. One possible explanation for

this, accepting the basic premise that these scores represent a fairly unsupported group of people, is that given their all round improvement on other measures, less support has been solicited by the WinCode 7 from their limited resources of such support. More likely is the case that there has been little or no change in the relationships tapped by this measure.

Table No. 26

INTERVIEW SCHEDULE FOR SOCIAL INTERACTION(I.S.S.I.)

	Phase 1.		Phase 7.		Australian pop. norms	
	\bar{X}	SD	\bar{X}	SD	Married \bar{X}	Divorced \bar{X}
AVAT	5.23	2.04	5.21	2.21	6.9	5.1
ADAT%	60.02	28.8	61.13	28.10	80.6	68.6
AVSI	7.20	3.34	8.01	3.91	9.4	9.9
ADSI	9.80	3.83	11.03	4.53*	12.8	11.3

* T-test $P < 0.05$ 1 tailed DF = 60

These scores supplement the results observed on the SBAS measure. The availability of attachment relationships (AVAT), and the percentage adequacy scores (ADAT%) show no statistically significant changes. This is entirely commensurate with a commonsense view of things, at least insofar as the Availability of Attachments score is concerned. Intimate attachment relationships are not formed within the time-scale under consideration. Indeed one might go so far as to say that within the context of the recovery programme,

individuals would possibly be discouraged from forming new, close attachment relationships during their treatment, as opposed to the type of relationship measured by the social integration scales. The two scales measuring wider relationships and acquaintances, and social contacts do provide evidence of a shift in the desired direction. The availability of social integration scale (AVSI), although not significant at the conventional confidence level, does provide a probability figure of $P < 0.108$ one-tailed. This is indicative of a shift in the desired direction, particularly when seen in conjunction with the adequacy of social integration (ADSI) scale. This latter does achieve significance at the .05 confidence level. These two scales point to an expansion in social relationships, perhaps the early stages of the development of a wider social network, usually a desired therapeutic goal. The test norms for the highest (married) and the lowest (divorced) scoring groups are displayed alongside the test results for comparison. Overall, the WinCode 7 continue to present a poorer profile than the divorced group, but change is in evidence and given the time-span over which these scores obtain perhaps this level of improvement is what one ought to expect. If the sample were sub-divided between married and unmarried, the AVAT score could no doubt be improved upon since in the Australian data marriage accounted for 23.8% of the variance of the AVAT scale and 3.4% of that of the AVSI scale.

Table No. 27

AFFECT BALANCE

Phase 1.			Phase 7.		
Domains	\bar{X}	SD	\bar{X}	SD	
Family life	4.48	1.78	5.16	2.06	
Marriage	3.62	2.65	3.80	2.92	
Neighbourhood	4.97	1.21	5.27	1.51	
Parents	3.21	2.74	3.37	2.90	
Work/unempl.	3.67	1.88	4.32	2.02	*
Finan. Sitn.	4.00	1.43	4.68	1.46	*
Health	4.92	1.49	5.63	1.30	**
Friendships	4.80	1.38	5.40	1.30	*
Housing	4.50	1.95	5.11	1.64	*
Religion	3.07	2.37	3.03	2.82	
Leisure	4.41	1.48	5.06	1.66	*
Organizations	2.79	2.63	3.60	2.85	*
Legal sitn.	2.33	2.54	2.11	2.72	
<u>Total</u> = 50.75			56.62		*
* = p < 0.05 ** = p < 0.005 60 DF 1 tailed					
postot4 - negtot			postot4 - negtot		
\bar{X} 12.98	3.56		\bar{X} 17.11	1.84	P < 0.016
SD 5.18	4.03		SD 6.94	3.74	
netot4			netot4		
\bar{X}	SD		\bar{X}	SD	
9.43	8.48		15.27	9.45	P < 0.001

Eight of the Affect Balance domains show significant changes in a positive direction. Of the five that do not only marriage runs counter to an intuitive or common sense expectation. The other domains that fail to reach significance were either not perceived as being important and tended not to be endorsed as frequently as others (legal situation, parents and religion fall into this category, at least for the majority of the sample) or were in domains like neighbourhood which are outwith the influence of the individual. In this respect it is interesting that housing does reach significance. Here it might be supposed that as individuals work and financial situation improves, particularly because they cease spending on alcohol, more of their income is available to improve their homes. However as indicated in the last chapter what is important for Affect Balance is the magnitude of change not how it is achieved. A structural consistency is assumed at least for purposes of calculations in the instrument's present format (this is not an argument against weighing domains via importance ratings, but this has not been done in the research presented here). From the calculation that is displayed in the table it is noted that an increase in positive totals between phases one and seven results in a significantly high Balance Score (Netot 4). What one is confronted with in this measure is a list of thirteen life domains or concerns, which collectively are hypothesized to encapsulate an individual's social existence. (The role domain "Retirement" was deliberately omitted because it was assumed correctly that the sample would contain very

few retired individuals). The Delighted - Terrible Scale taps a subjective perception of the person's well-being which cumulatively produces an assessment of the person's quality of life. The assessment necessarily involves a cognitive evaluation and an affective ingredient, the emphasis being on the affective aspect, that is a feeling state either positive or negative that things are either getting better or worse. It is evident from the results above that the Wincode Sevens feel that life as a whole is substantially better for them at phase seven than it was at phase one.

SUMMARY OF SOCIAL VARIABLES

The variables in this section display similar improvements to drinking and psychological variables. An inspection of the Social Behaviour Assessment Schedule Means for Disturbed Behaviour and consequent Burden experienced by the individuals concerned, indicates a substantial reduction in both physical and mental problems. It was noted "Burden" rating had shrunk to a level that might be hypothesized to reflect the level of such experiences in the general population.

With regard to existing supportive relationships, it appears to be the case that little change is in evidence on this measure. The emotional and practical support scales show no improvement. These scales focus on family and neighbours. It is also seen that the Affect Balance domain for marriage remains unchanged. (This is unremarkable given the time scale of the study and will be commented on below). A better measure of social relationships outwith the family is the

Interview Schedule for Social Interaction, Adequacy of Social Integration Scale (ADSI). Like the Social Behaviour Assessment Schedule the scales that reflect spousal type relationship show no change i.e. AVAT and ADAT %. However the Adequacy of Social Integration Scale (ADSI) shows a statistically significant change in the desired directions. This scale measures an individual's appraisal of his social relationships generally. It seems that the completers are getting more from the social contacts they already possess. A trend toward increased social integration is in evidence in the availability scale (AVSI). It is therefore reasonable to conclude that in the sphere of social integration there are very definite improvements with the beginnings of an expansion of socially supportive networks and greater satisfaction with existing relationships. Although it is not possible to set Windcode 7 against an appropriate comparison group for social integration, it is probable that they still fall somewhat behind what one might expect to observe in the general population; however given the degree of impairment observed initially in the sample, the achievements are quite marked. They are in fact moving quite close to the Australian norms for the general population for this instrument.

In addition to improvements in social relationships the Wincode 7 group exhibit multiple improvements in the life domains measured by the Affect Balance scale such that the overall reporting of subjective quality of life is greatly improved.

The Affect Balance score has two aspects to it.

First it supplies a summary score of an individual's current state of wellbeing which is a composite of the general evaluations of the thirteen life domains. The assessment of each particular life domain or concern will vary across individuals in respect of relevance and importance, but the balance score reflects the same mechanism across the sample regardless of idiosyncrasies. The Affect Balance measure also registers change both in particular domains and overall balance score (Netot 4). As noted already the completers show significant gains across a variety of domains, those which are within their power to influence, with a consequent improvement in Balance Score.

The second aspect of Affect Balance relates to a theoretical issue, in that it is presumed to relate to a higher order factor; an overall evaluation of the quality of life or wellbeing or happiness. It describes a state, it is "a state of being" concept. As such it is a complicated judgement about life in respect of relevant domains appraised in terms of personal history and social comparison. Although the data are not presented in this thesis, an overall rating of happiness was collected as part of the assessment battery. This type of avowal of overall Happiness has been found to correlate highly with scales of positive and negative affect (Bradburn 1967, Bradburn and Caplovitz 1965, Campbell 1976, Moriwaki 1974, Andrews and Withey 1976, Campbell Converse and Rodgers 1976, Cantril 1965). This creates an empirical link of a linear sort between life domains and Affect Balance, but as Bradburn and others have demonstrated the positive and

negative scales form two independent clusters and are not negatively correlated, suggesting two independent dimensions as opposed to a continuum; yet both measures, positive and negative affect, correlate with or predict avowals of global happiness. The finding of independence between measures of positive and negative affect and the relationship of each to ratings of global satisfaction is of considerable importance for a Theory of Affect Balance.

What we observe in the Wincode 7 Affect Balance scores are not only an additive measure of satisfaction or dissatisfaction with life domains but also a rating of a "Feeling State" which is qualitatively different or at least is presumed to be so and as noted previously is underpinned by empirical support. There is also a change in language from empirical concepts to a priori concepts to describe these issues. This suggests that mechanisms are at work which are not immediately self evident. One might say that there is a qualitative change in the material under consideration. The summary Affect Balance score equates with the generalized rating of happiness. An individual's experience of life as a whole is coloured by the hedonic tone implied by the global rating. (This clearly has implications for a theory of emotion).

The completers in this light can be seen not only as objectively improving their life domain situation but, by virtue of this, to have so disposed themselves to evaluate their situation in a more optimistic fashion.

In talking about this second aspect of Affect Balance

it might be noted that there is an implied threshold beyond which the sample pass to view their worlds in a more optimistic manner, a demarcation between various levels or states of wellbeing which may or may not correlate precisely with test scores. The sample at phase one presented a picture of much dissatisfaction with life as well as impaired psychological and social profiles. At phase seven they had moved beyond that state to one which may be considered very close to normal or desirable. In this respect Maslow's hierarchy of needs might explain the upward progression through thresholds of needs (Maslow 1972). Initially survival needs might be assumed to have driven the sample into treatment - survival in the sense of family or work, etc. Then social needs followed by psychological needs have to be gratified. The data presented so far provides evidence of progression through such a hierarchy, perhaps the passage from one level to another entails a qualitative shift in thresholds as imagined above.

Such a view of changing perceptions within the sample with increasing Affect Balance would be set against a continuum model of increasing happiness or indeed a discontinuous model of another sort which does not depend on a linear progression through a hierarchy of needs presumed to be constant for everyone. Adaptation-Level Theory (Helson 1964) has been employed to explain the sort of data under consideration (Brickman and Campbell 1971). Here a more cognitive approach is presumed: the significance of inputs in the form of environmental contexts which are evaluated as life

domains. The subjective experience of life domains is not entirely a function of the magnitude of changes in the environment but of judgements made by individuals between historically changed states: that is, the discrepancy between one state of affairs, say financial situation or friendships at the present time and some period in the past. In respect of happiness the pleasantness or aversiveness of circumstances are not to be regarded as objective and concrete but are relative to past experience. This view admits of the notion of habituation. Individuals may habituate themselves to circumstances so that they feel either less or more happy by virtue of a process of habituation. For instance Brickman found that lottery winners came to devalue routine pleasures. The point about Adaptation Level Theory is that time related self evaluations of life domains can provide a sort of explanation for the second aspect of Affect Balance which the completers are thought to have benefited from. Detailed discussion of the theoretical issues involved do not belong in this section nor is it relevant to consider issues of habituation or "hedonic treadmills" in the context of the present sample since these are difficulties which they are presumed not to have encountered at the stage of their recovery careers which is the focus of this thesis. The point to be emphasised is that the Affect Balance score indicates a quality to do with hedonic tone which influences judgements and the way the sample view their lives.

To conclude this section the sample might be said to have made major improvements in the quality of their lives.

GENERAL CONCLUSION

Taking Wincode 7 as satisfactory outcome, 36.7% of the sample completed treatment over the study period. As noted previously this approximates to the rule of one third much commented upon in the literature. On almost all variables studied the sample of completers achieved marked improvement such that they, in most instances, provided a picture of a trouble free group. Those variables, like marriage, which do not show marked improvements are explained by reference to clinical expectations and the time scale involved. Others like general social relationships do provide evidence of change and an optimistic prognosis.

Chapter 5 References

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CHAPTER 6

DATA ANALYSIS III

In this chapter the data analysis will explore change across the study period with a special focus on the relationship between Affect Balance and a selection of study variables. WinCode 7 will be looked at using scores from the beginning, middle and end of the study period. Change scores are generated for each phase to enable an overview of the pattern of change across the study period by converting mean scores to Z scores to facilitate comparison between tests. Change scores also assist with the interpretations of correlations which were also calculated for completers at phases 1,3 and 7. The relationship between Affect Balance and other scores is further explored by means of multiple analysis of variance: MANOVA (SPSSX/PC). An analysis of covariance, at phases one, three and seven, provides a more fine grained analysis of Affect Balance and its relationship to other study variables. A correlation analysis of within subject change scores for drinking and social variables is provided for phases one and seven, these being the only two phases for which these data are available.

Finally stepwise multiple regression was employed to predict six nominated outcome variables at phase one (B1), to assess the efficacy of Affect Balance in predicting outcome. Five of these variables are conventional outcome criteria and the sixth is the amount of treatment received.

Table No. 28

Table of Change Scores for Psychological Variables Nettot 4
and DRIE

Var.	B1	P1	P2	P3	P4	P5	B2
	2 - 1	3 - 2	4 - 3	5 - 4	6 - 5	7 - 6	
Nettot	\bar{X} 2.48	1.21	0.41	-0.66	0.31	2.10	
4	S.D. 6.50	5.02	5.53	4.32	6.28	5.31	
DRIE	\bar{X} -5.00	-0.66	-0.20	-0.18	-0.30	-0.39	
	S.D. 5.97	3.09	3.68	4.32	3.35	3.52	
C/C A.	\bar{X} -1.33	-0.72	-1.33	0.20	-0.75	0.62	
	S.D. 3.00	2.97	2.75	2.15	2.51	2.58	
P.	\bar{X} -0.87	-0.48	-0.26	-0.21	-0.18	-0.13	
	S.D. 2.12	2.24	1.62	1.45	1.48	1.42	
O.	\bar{X} -0.72	-0.31	-0.16	0.03	-0.18	-0.16	
	S.D. 2.94	2.77	2.50	1.93	1.53	1.91	
S.	\bar{X} -2.44	-0.31	-0.41	0.13	-0.11	-0.21	
	S.D. 3.12	2.36	2.00	1.65	1.47	1.63	
D.	\bar{X} -1.75	-0.13	-0.74	-0.23	-0.38	-0.39	
	S.D. 2.64	3.18	2.88	2.52	2.19	1.56	
H.	\bar{X} -0.95	-0.11	-0.48	-0.31	0.16	-0.33	
	S.D. 3.41	2.60	2.29	2.16	1.77	1.94	
*S/E	\bar{X} 6.23	7.34	4.82	-0.44	4.21	2.66	
	S.D. 16.98	16.89	14.68	14.63	14.24	13.08	

C/C = Crown Crisp Inventory.

*S/E = self-esteem

An inherent problem connected with change scores, or difference scores as they are sometimes called, is that an element of serial dependency is involved in their use. A consequence of this is that they tend to register larger

magnitudes of change at the beginning of a series of repeated measures. The magnitude of such change tends progressively to diminish across the time period as increasing amounts of the scores measured are already accounted for and hence, less are available to be measured. This is a purely arithmetical fact of change scores and applies regardless of the nature of the measure being recorded.

To consider the pattern of change in Affect Balance first (nett4), we note that the first change score ($B_1 - P_1$, $2 - 1$), and the last one ($B_2 - P_5$, $7 - 6$), register the largest amounts of change. Between these two scores, increases in Affect Balance scores occur with decreasing magnitude until the $5 - 4$ score when there is a change of sign indicating that Affect Balance actually drops at this point by a moderate amount, -0.66 . Thereafter it begins to increase again. The overall tendency appears to be one of fairly substantial initial increase tailing off to a trough in the phases 3 and 4 and rising again at the end of the study period. The drop in score at change score $5 - 4$ is underscored by a tightening up of the standard deviation for that score. The reversal of trend at $5 - 4$ is preceded by a marked reduction in score at $4 - 3$. It appears that this group of completers experience something of a trough in their reported quality of life at this period which lasts for two months between the third and fourth month of the treatment period, and begins to pick up again in the fifth, showing good progress by the seventh month. Both in the data presented earlier on attrition, and in the folklore of the treatment

programme itself, this three month point is considered to be a particularly difficult one for the individuals on the programme. The reasons for this are variously ascribed by programme staff. The issue in fact is inexplicable in terms of the data collected for this thesis. A common explanation is that it relates to the slope of the learning curve, given that it necessarily relates to a programmatic issue, or at least a person-situation (programme) interaction, because the 61 individuals involved did not all encounter this phase at the same time, but over an extended time period. A likely event that most of WinCode7s would have in common at this stage, is a change of therapeutic group. This is always a disruptive event in a person's recovery career: a new group leader, perhaps new group members, and a breaking of some old bonds which had been forged in an emotionally turbulent climate.

Drinking related locus of control exhibits the same pattern of change as does nettot4. DRIE begins with a large reduction in score at the 2 - 1 score. A score of 5 accounts for 58% of the total initial score for DRIE (8.25) for WinCode 7. The next largest change score for DRIE is 0.16 at 3 - 2. Beyond phase 3, little change is left to be accounted for. This sort of dramatic initial change in DRIE is explicable in terms of early treatment experiences which, to a significant degree, are aimed precisely at having this sort of effect on the types of attitude and perceptions measured by DRIE.

The overall pattern of change among the Crown Crisp scales are very similar to the pattern observed for nettot4,

including the trough at change score 5 - 4, and the general depression at phases 3 and 4. Like the other measures, the majority of the largest changes occur between B1 and P1 (2 - 1). The next largest change for the Crown Crisp scales tend to be evenly distributed between 3 - 2, and 4 - 3 change scores. At 5 - 4 there is a marked increase in all scales coinciding with the drop in nettot4. Three scales actually change sign at 5 - 4, Free Floating Anxiety, Depression and Somatic concern register an increase against the prevailing downward trend. The pattern then is one of gradual decrease until phase 4, then a slight increase or levelling off of scores occurs before the gradual decrease is resumed during the last two phases.

The self-esteem measure shows a similar pattern to the rest but in the reverse direction: a gradual increase over the phases with the exception 5 - 4, when change is actually reversed with a change score of -0.44.

A trend is observed in these change scores. This is demonstrated by the trend tests listed below. These are SPSSX procedures involving averaged tests of significance across the seven phases on raw score data.

Table Number 29

Trend Tests for Psychological Variables and Nettot4 Change Scores

* * ANALYSIS OF VARIANCE -- DESIGN 1 * *

Variable NETTOT4

Tests involving 'TRIAL' Within-Subject Effect.

AVERAGED Tests of Significance for PHASE using UNIQUE sums of squares

Source of Variation	SS	DF	MS	F	Sig of F
WITHIN CELLS	8641.58	360	24.00		
TRIAL	1164.70	6	194.12	8.09	.000

* * ANALYSIS OF VARIANCE -- DESIGN 1 * *

Variable DRIE

Tests involving 'TRIAL' Within-Subject Effect.

AVERAGED Tests of Significance for PHASE using UNIQUE sums of squares

Source of Variation	SS	DF	MS	F	Sig of F
WITHIN CELLS	4017.67	360	11.16		
TRIAL	2133.47	6	355.58	31.86	.000

* * ANALYSIS OF VARIANCE -- DESIGN 1 * *

Variable ACC

Tests involving 'TRIAL' Within-Subject Effect.

AVERAGED Tests of Significance for PHASE using UNIQUE sums of squares

Source of Variation	SS	DF	MS	F	Sig of F
WITHIN CELLS	1735.58	360	4.82		
TRIAL	824.42	6	137.40	28.50	.000

* * ANALYSIS OF VARIANCE -- DESIGN 1 * *

Variable PCC

Tests involving 'TRIAL' Within-Subject Effect.

AVERAGED Tests of Significance for PHASE using UNIQUE sums of squares

Source of Variation	SS	DF	MS	F	Sig of F
WITHIN CELLS	744.85	360	2.07		
TRIAL	204.87	6	34.14	16.50	.000

* * ANALYSIS OF VARIANCE -- DESIGN 1 * *

Variable OCC

Tests involving 'TRIAL' Within-Subject Effect.

AVERAGED Tests of Significance for PHASE using UNIQUE sums of squares

Source of Variation	SS	DF	MS	F	Sig of F
WITHIN CELLS	1230.94	360	3.42		
TRIAL	92.78	6	15.46	4.52	.000

* * ANALYSIS OF VARIANCE -- DESIGN 1 * *

Variable SCC

Tests involving 'TRIAL' Within-Subject Effect.

AVERAGED Tests of Significance for PHASE using UNIQUE sums of squares

Source of Variation	SS	DF	MS	F	Sig of F
WITHIN CELLS	1199.91	360	3.33		
TRIAL	498.95	6	83.16	24.95	.000

Variable DCC

* * ANALYSIS OF VARIANCE -- DESIGN 1 * *

Tests involving 'TRIAL' Within-Subject Effect.

AVERAGED Tests of Significance for PHASE using UNIQUE sums of squares

Source of Variation	SS	DF	MS	F	Sig of F
WITHIN CELLS	1602.63	360	4.45		
TRIAL	535.65	6	89.28	20.05	.000

* * ANALYSIS OF VARIANCE -- DESIGN 1 * *

Variable HCC

Tests involving 'TRIAL' Within-Subject Effect.

AVERAGED Tests of Significance for PHASE using UNIQUE sums of squares

Source of Variation	SS	DF	MS	F	Sig of F
WITHIN CELLS	1407.45	360	3.91		
TRIAL	176.55	6	29.43	7.53	.000

* * ANALYSIS OF VARIANCE -- DESIGN 1 * *

Variable Self_Est

Tests involving 'TRIAL' Within-Subject Effect.

AVERAGED Tests of Significance for PHASE using UNIQUE sums of squares

Source of Variation	SS	DF	MS	F	Sig of F
WITHIN CELLS	58499.78	360	162.50		
TRIAL	28757.07	6	4792.85	29.49	.000

Other process variables were included from P1 onwards.

These were a Short Social Adjustment Scale, a Life Event Inventory, and three measures rating perceptions of the clinic programme for Relevance, Interest and Helpfulness. Feedback from programme participants and workers indicated that great difficulty arose with the administration of the life event inventory, and many subjects did not complete it at all. Some presumably did not take it seriously. The greatest difficulty appears to have been its length. The data problem with this measure is probably invalidity by virtue of non-completion as opposed to faking. However the Wincode 7 group had the highest rate of endorsement of the Life Event Inventory, again probably an indication of their serious-mindedness in respect of their programme involvement. The change scores for these variables are available only for 3 - 2 to 6 - 5.

Table No. 30

CHANGE SCORES, SOCIAL AND PROGRAMME VARIABLES

	P1	P2	P3	P4	P5
var.	3 - 2	4 - 3	5 - 4	6 - 5	
ssadj \bar{X}	2.72	-0.66	-0.80	3.00	
S.D.	16.18	12.06	14.64	13.86	
life + \bar{X}	-0.43	-0.23	-0.44	-0.33	
evts. S.D.	3.25	2.33	2.29	1.81	
- \bar{X}	-0.61	-0.28	0.13	-0.15	
S.D.	2.40	1.85	1.81	1.68	
tot. \bar{X}	-1.03	-0.51	-0.18	-0.61	
S.D.	4.63	2.83	2.40	2.88	
CXC* int. \bar{X}	-4.20	6.23	-0.79	3.30	
S.D.	32.86	27.16	20.16	19.77	
rel. \bar{X}	2.97	-2.00	-1.82	3.98	
S.D.	20.20	17.24	24.12	17.58	
help. \bar{X}	-7.52	8.44	-0.92	3.98	
S.D.	31.53	24.46	75.76	17.34	

 ssadj = short social adjustment scale

tot = total Life Events

*Charing Cross Clinic Scales

int = Interest

rel = Relevance

help = Helpfulness

The change scores for the short social adjustment measure show an initial increase of moderate size followed by two decreases of smaller magnitude. Again, these decreases were over phases 3 and 4 of the treatment period. They

increase again more substantially at the end of the study period leaving an overall increase in score. The Standard Deviation indicates a fairly squat distribution of scores. Generally, the mean at the beginning P1 is quite high (76.0) and 80 at B2 is also quite large. The most significant feature of this pattern of scores is the trough around the three month period which makes it look like the other change scores discussed.

The next scores in the table are those of the Life Event Inventory. As mentioned above, grave doubts have to be entertained concerning the validity of this measure. However, a pattern does emerge which, at least in one important respect imparts validity insofar as a comparison with the other scores is concerned. This feature is the increase in negative events for change score 5 - 4 amid a series of decreasing scores. The most notable features of these scores are the overall reduction in the reporting of life events, both positive and negative, and the extremely tight Standard Deviation indicating either a limited range of reporting, and/or few individuals contributing to this reportage, most probably the latter. As already noted, the only increase observed is for change scores 5 - 4, negative events.

The last set of scales in the table is the one measuring the respondents' view of the treatment programme in terms of interest, relevance and helpfulness. These results are difficult to interpret. They produce a pattern of alternative positive and negative ratings with the positive scores, when they occur, having the greatest magnitudes, such

that the overall score registers an increase on all three scales. The relevance scale seems curiously independent of the other two scales. A prominent feature of these results is the wide spread of scores as indicated by the standard deviation. Clearly, opinions varied considerably about the value of the programme as judged by these scores.

An examination of the change scores indicates a fairly generalized pattern of change where the gradient of change starts off steep and levels out, taking a dip at change score 5 - 4. This is representative of a slowing down of change at phases 3 and 4; in other instances, what is observed is a brief reversal of trend.

Because a primary focus of this thesis is on the concept of Affect Balance and its behaviour in respect of other variables, it is tempting to see the disruption at change score 5 - 4 as Affect Balance influencing all the other scores, but such an explanation is not tenable since no more than ten individuals would have entered this phase at the same time. A much more likely explanation, as noted above, is that the major determinant of the effects observed at that point in the study period, will have been a programmatic variable. It is noteworthy that the clinic relevance scale takes a decrease at change score 4 - 3, just prior to the more general disruption of scores, as does the short social adjustment scale which, in fact includes a question about the individual's progress in the clinic programme.

With regard to the overall trend across the phases, these social and programme process variables are much less

active than the nettot4, DRIE and Crown-Crisp scales displayed in the previous table of change scores. The trend tests listed below indicated that an overall trend is observed only for the Life Event Scales and the Charing Cross Clinic Help Scale. The Short Social Adjustment Scale and the Charing Cross Clinic scales for Interest and Relevance fail to exhibit a trend.

Table No 31

Trend Tests for Social and Programme Change Scores

* * ANALYSIS OF VARIANCE -- DESIGN 1 * *

Variable Soc adj

Tests involving 'TRIAL' Within-Subject Effect.

AVERAGED Tests of Significance for PHASE using UNIQUE sums of squares

Source of Variation	SS	DF	MS	F	Sig of F
WITHIN CELLS	26827.79	240	111.78		
TRIAL	620.21	4	155.05	1.39	.239

* * ANALYSIS OF VARIANCE -- DESIGN 1 * *

Variable Plus Life Events

Tests involving 'TRIAL' Within-Subject Effect.

AVERAGED Tests of Significance for PHASE using UNIQUE sums of squares

Source of Variation	SS	DF	MS	F	Sig of F
WITHIN CELLS	972.90	240	4.05		
TRIAL	76.30	4	19.07	4.71	.001

* * ANALYSIS OF VARIANCE -- DESIGN 1 * *

Variable Minus Life Events

Tests involving 'TRIAL' Within-Subject Effect.

AVERAGED Tests of Significance for PHASE using UNIQUE sums of squares

Source of Variation	SS	DF	MS	F	Sig of F
WITHIN CELLS	584.34	240	2.43		
TRIAL	33.66	4	8.41	3.46	.009

* * ANALYSIS OF VARIANCE -- DESIGN 1 * *

Variable Total Life Events

Tests involving 'TRIAL' Within-Subject Effect.

AVERAGED Tests of Significance for PHASE using UNIQUE sums of squares

Source of Variation	SS	DF	MS	F	Sig of F
WITHIN CELLS	1941.52	240	8.09		
TRIAL	186.08	4	46.52	5.75	.000

* * ANALYSIS OF VARIANCE -- DESIGN 1 * *

Variable cxc interest

Tests involving 'TRIAL' Within-Subject Effect.

AVERAGED Tests of Significance for PHASE using UNIQUE sums of squares

Source of Variation	SS	DF	MS	F	Sig of F
WITHIN CELLS	90357.57	240	376.49		
TRIAL	2518.83	4	629.71	1.67	.157

* * ANALYSIS OF VARIANCE -- DESIGN 1 * *

Variable cxc Relevance

Tests involving 'TRIAL' Within-Subject Effect.

AVERAGED Tests of Significance for PHASE using UNIQUE sums of squares

Source of Variation	SS	DF	MS	F	Sig of F
WITHIN CELLS	64774.85	240	269.90		
TRIAL	765.55	4	191.39	.71	.586

* * ANALYSIS OF VARIANCE -- DESIGN 1 * *

Variable cxc Help

Tests involving 'TRIAL' Within-Subject Effect.

AVERAGED Tests of Significance for PHASE using UNIQUE sums of squares

Source of Variation	SS	DF	MS	F	Sig of F
WITHIN CELLS	71132.72	240	296.39		
TRIAL	4389.28	4	1097.32	3.70	.006

CORRELATION ANALYSIS

The correlation matrix generated for this part of the analysis is to be found in appendix "A".

Drinking Variables

The drinking related variables included in this analysis are the Drink Related locus of control measure (DRIE) and Severity of Dependence. These two variables did not correlate with each other. There are no particular reasons why they should since they tap distinctly different conceptual domains. The Severity of Dependence Scale was only administered at phases 1 and 7.

Severity of Dependence correlated at phase 1 with the four Crown Crisp Scales, Free Floating Anxiety, Phobic Anxiety, Somatic Concern and Depression at a high level of significance ($P < 0.001$), and with Self-Esteem and Affect Balance (nettot 4), again at the $P < 0.001$ level. Exactly the same pattern of correlation is to be observed at phase 7. These correlations present no great surprises and might have been predicted on rational grounds.

DRIE has fewer correlations at phase 1, only the Somatic Concern scale of the Crown Crisp Inventory and Self-

Esteem. Perhaps one might have expected DRIE to have been related to a wider range of psychological variables at this stage. By phase 3 however, DRIE correlated with 5 Crown Crisp scales, all at high levels of significance.

At the final phase, DRIE correlated significantly with the Phobic Anxiety, Somatic Concern and Depression scales and also with Self-Esteem. The DRIE pattern of correlation, unlike Severity of Dependence, looks quite complex. It correlates with few variables initially, then, at the mid-point of the study its range of correlations increases and then diminishes slightly at the end-point. Possibly the very high initial DRIE score masks its relationship with other variables. The score dropped dramatically and almost immediately to much the same level it finished at. The increase in the number of correlations at phase 3 probably reflects the general, uniform trend in scores at that phase brought about by a levelling off of scores seen in the change scores for phases 3 and 4. It changed little from phase 5 and this may account for the reduction of correlations between phases 3 and 5.

THE PSYCHOLOGICAL VARIABLES

The pattern of correlation amongst the Crown Crisp variables remains fairly constant throughout. The Crown Crisp scales all correlate with each other throughout phases 1,3 and 7, except for hysteria. The Hysteria scale correlates at phase 3 only with Free Floating Anxiety, Phobic Anxiety, Depression and Obsessionality scales and with no other variables at any time. Again, we notice the disruption apparent at phase 3

in the behaviour of the Hysteria scale.

Self Esteem - This variable deploys the most stable pattern of correlations across the three phases. It correlates with most other variables, notable exceptions being the Obsessionality (obs) and Hysteria (hys) scales at phase 1. By phase 3 it correlates with the obsessionality scale, but not with the hysteria scale. Throughout, it correlates with Affect Balance. Again an increase in correlation is observed at phase 3.

THE LIFE-EVENT AND CHARING CROSS CLINIC PROGRAMME SCALES

These scales correlate only with themselves, except for the relevance scale which correlates also with Affect Balance (Nettot 4). The fact that these three clinic evaluation scales failed to correlate with other variables at any place throughout the study period, and also the fact that they behaved somewhat eccentrically amongst themselves, cast doubt upon their validity.

AFFECT BALANCE

Table 32 displays the correlations for Affect Balance (nettot 4) for the three phases under consideration.

TABLE No. 32

Correlations for Affect Balances

	Phase 1.	Phase 3.	Phase 7.
variables	nett4	nett4	nett4
Severe Dep.	-.3669 **	----	-.1937
DRIE	-.2778	-.3630 **	-.2189
FFA	-.4997 **	-.3669 **	-.4300**
Pho	-.2421	-.2094	-.2689
Obs	-.1411	-.0600	-.1510
Som	-.3729 **	-.2836	-.3909**
Dep	-.4005 **	-.2553	-.5811**
Hys	.0080	.1563	.1356
Self-Esteem	.3756 **	.4736**	.6412**

** $P < .001$

The pattern of correlations for nett4 across the three phases indicates a change in the range of correlations at phase 3. Unlike the psychological variables, nett4 decreases in the number of correlations observed. Again, this indicates erratic performance in respect of phase 3.

The reason for Severity of Dependence and DRIE not correlating with nett4 at phase 7 has already been noted. The significant correlations observed are for the most part in the low to moderate range of significances. The key variables to emerge from the matrix for nett4 are the Crown Crisp Scales and Self-Esteem. The most telling feature of the matrix is that the two Crown Crisp Scales, Somatic Concern and Depression drop out of contention during phase 3. Affect Balance is of course positively correlated with most variables

measured, hence, when it levels out or drops it tends to lose its correlations. This happened at phase 3. However, as noted earlier, one is not justified in assuming that the odd behaviour of scores at phase 3 is related to the performance of Affect Balance at this phase. Probable explanations have already been suggested and these concern programmatic events. Perhaps if it is the case that individuals in recovery programmes tend to experience difficulty at around the three to four month period, regardless of treatment, as much therapeutic folklore suggests, then it is conceivable that two processes are at work around phase three, one related to disjunctions in the individual's recovery career in the programme, the other, to difficulties in his daily life which are picked up by Affect Balance. The correlation analysis points to both continuous and interrupted relationships of Affect Balance, with the scores of other variables across the study period. A variety of measures appear to mimic nettot 4, which raises the question of its importance for other variables.

MULTIPLE ANALYSIS OF VARIANCE

The analysis is continued with MANOVA to explore further, within the limitations of the design, any direct relationship between Affect Balance and other variables. For this purpose, a null hypothesis is established, namely, that Affect Balance is not a factor in other scores. This is so because the analysis moves at this point from a descriptive mode to an experimental mode, in that the hypothesis-testing capabilities of MANOVA are a form of statistical

experimentation. This is as far as it is possible to get with experimentation within the design. The hypothesis is tested by multiple analysis of variance. To utilize MANOVA procedures on SPSSX/PC, some recoding of data was required to meet the specifications of the tests in MANOVA, the main criterion to be met being that there should be no missing values. The range of nettot4 scores were inspected and the values -2, 5 were taken as being representative of the distribution of scores. A new variable, newab4 was generated with the formula $NEWAB4 = (NETTOT4/5)INT$, an integer being required by the statistical procedure involved. Newab4 becomes the new Affect Balance score suitable for inclusion in the MANOVA procedure as an independent variable.

MANOVA was performed on a selection of WinCode 7 variables. Drinking variables were tested at phases 1 and 7, these being the only two phases for which data was available. Psychological variables were treated at phases 1,3 and 7 and social variables were examined at the beginning and end of the study period since, like drinking variables, these were the only two periods for which data was available. Tables produced by the MANOVA procedure can be found in appendix B.

Drinking Variables

To treat the drinking variables first, the second order Alcohol Use Inventory Scales and Severity of Dependence were included in the analysis at phases 1 and 7. An inspection of the table in appendix B indicates that the Pillais trace, the preferred test because of its greater robustness, is not significant. However, the Hotellings and

Wilkes Multivariate Tests of Significance are. These tests are measuring different aspects of the individual variables as a composite variable. The tests indicate that only scale D2 approaches significance at 0.09 at phase 1, and at phase 7 there are no significant effects observed. This is more or less what one would expect, certainly at phase 7. There is no reason to expect a relationship between the 2nd order AUI scales and Severity of Dependence and newab4 since they are in the main descriptions of drinking style and its consequences. In this respect, the highest F test figure is recorded for scale D2 which is a measure of alcohol use deterioration and may have a slight relationship with quality of life issues. Generally though, one may conclude that the hypothesis, that newab4 is not related to the drinking variables in a direct way, has not been refuted by these tests.

To explore the relationships between the Affect Balance (Nettot4) and the drinking variables from the perspective of change, a correlation matrix was generated using within-individual change scores.

Table No. 33

Correlation Matrix for Drinking Variables and Nettot4 Based on Change Scores

CORRELATIONS /VARIABLES A ALC US TO NETTOT4.

Correlations:	A ALC US	B ALC US	C ALC US	D1 ALC US
A ALC US	1.0000	.3653*	.4520**	.3939**
B ALC US	.3653*	1.0000	.6617**	.5325**
C ALC US	.4520**	.6617**	1.0000	.6164**
D1 ALC US	.3939**	.5325**	.6164**	1.0000
D2 ALC US	.4473**	.5217**	.5920**	.8223**
G ALC US	.4125**	.7107**	.7801**	.7968**
SEV DEP	.0676	-.0188	.1156	.0514
NETTOT4	-.1883	-.0459	-.1047	-.0424

Correlations:	D2 ALC US	G ALC US	SEV DEP	NETTOT4
A ALC US	.4473**	.4125**	.0676	-.1883
B ALC US	.5217**	.7107**	-.0188	-.0459
C ALC US	.5920**	.7801**	.1156	-.1047
D1 ALC US	.8223**	.7968**	.0514	-.0424
D2 ALC US	1.0000	.7221**	.1949	-.1292
G ALC US	.7221**	1.0000	.0332	-.1674
SEV DEP	.1949	.0332	1.0000	-.1328
NETTOT4	-.1292	-.1674	-.1328	1.0000

N of cases: 61 1-tailed Signif: * - .01 ** - .001

When these within-individual change scores are correlated it is observed that nettot4 does not correlate with any other variables. The results of the multivariate analysis are extended by this result which deals with nettot4's relationship to other scores in terms of magnitude of change. The correlation of change scores and the previous multivariate analysis of variance at two separate points in time deal with two functionally discrete aspects of the behaviour of nettot4.

As one would predict, changes across the Alcohol Use Inventory Scales all inter-correlate, the majority at a high level of significance. This indicates fairly uniform change by individuals across these scales. The Severity of

Dependence change scores would not be expected to perform in the same manner because, as noted previously, this measure is somewhat anomalous for individuals who are survivors in an abstinence oriented recovery programme. Most would have completed this measure as they did for phase one since it requests them to consider their last typical month of drinking.

PSYCHOLOGICAL VARIABLES

The variables included here were the Crown Crisp Experiential Index Scales and Self-Esteem. A scrutiny of the relevant table for these variables at phase 1 indicates that the Pillais trace and other multivariate tests all have significant F values. An inspection of the univariate tests indicates that most of the Crown Crisp variables are highly significant, the exceptions being the scales for Obsessionality and Hysteria. Self-Esteem also failed to achieve significance at this point. The table of test results for phase 3 shows that higher order tests are all significant. The table of univariate tests indicates that, at this third phase, in addition to the two variables that were not significant, a further variable, phobic anxiety, ceases to achieve significance. However, Self-Esteem becomes highly significant at this phase.

At phase 7 it is noted that the Pillais trace does not reach significance but the Hotellings and Wilkes tests do. Three variables achieve significance on the univariate tests, the Somatic Concern and Depression scales of the Crown Crisp Inventory and the Self-Esteem measure. The Anxiety Scale also

achieves a significance level of 0.081 which approaches the somewhat arbitrary level of 0.05.

The results of these tests indicate what are two related aspects of the relationship between newab4 and the variables included. The Pillais trace and other multi-variate tests are multi-variate tests of significance which treat the variables included as a composite measure and indicate the probability of the null hypothesis as high or not. In all the significant instances of the multivariate tests noted above, the null hypothesis is rejected; that is, that Affect Balance, (newab4) does have an effect on the variables listed in the table. The lower part of the table shows the univariate F tests showing the individual ways in which the scales differ in relation to newab4. That is, the effect noted in the multi-variate tests is concentrated in the variables listed as shown in the univariate tests.

The precise amount of variance accounted for is not known. It is evident then that Affect Balance (newab4) does merit being regarded as a factor in the tests that achieve significance. The pattern of these psychological variable changes across the study period is of an increase in the number of variables achieving significance at phase 3 and a reduction at phase 7. Some of the variables hold up very strongly across the study period, particularly Crown Crisp Scales, Somatic Concern, Depression and Self-Esteem from phase 3.

The repeated measure design capability of MANOVA is utilized on the same three phases (one variable at a time) to further explore the relationships of affect balance (nett4)

and the Psychological variables, nettot4 being employed as the covariate.

Table No. 34

Analysis of Covariance of Affect Balance (Nettot4) with Psychological Variables

* * ANALYSIS OF VARIANCE -- DESIGN 1 * *

Tests of Significance for ACC_ using UNIQUE sums of squares
Source of Variation SS DF MS F Sig of F

WITHIN CELLS	2282.75	179	12.75		
REGRESSION	475.61	1	475.61	37.29	.000
CONSTANT	4598.44	1	4598.44	360.58	.000
PHASE_NO	311.58	2	155.79	12.22	.000

Regression analysis for WITHIN CELLS error term
Dependent variable .. ACC_

COVARIATE	B	Beta	Std.Err.	t-Value	Sig. of t
NETTOT4	-.18263	-.41524	.030	-6.107	.000
COVARIATE Lower -95% CL-					
NETTOT4	-.242	-.124			

* * ANALYSIS OF VARIANCE -- DESIGN 1 * *

Tests of Significance for PCC_ using UNIQUE sums of squares
Source of Variation SS DF MS F Sig of F

WITHIN CELLS	1401.39	179	7.83		
REGRESSION	84.29	1	84.29	10.77	.001
CONSTANT	1559.47	1	1559.47	199.19	.000
PHASE_NO	82.03	2	41.01	5.24	.006

Regression analysis for WITHIN CELLS error term
Dependent variable .. PCC_

COVARIATE	B	Beta	Std.Err.	t-Value	Sig. of t
NETTOT4	-.07688	-.23819	.023	-3.281	.001
COVARIATE Lower -95% CL-					
NETTOT4	-.123	-.031			

* * ANALYSIS OF VARIANCE -- DESIGN 1 * *

Tests of Significance for OCC_ using UNIQUE sums of squares
Source of Variation SS DF MS F Sig of F

WITHIN CELLS	1857.11	179	10.37		
REGRESSION	26.76	1	26.76	2.58	.110
CONSTANT	3271.30	1	3271.30	315.31	.000
PHASE_NO	46.97	2	23.48	2.26	.107

Regression analysis for WITHIN CELLS error term
Dependent variable .. OCC_

COVARIATE	B	Beta	Std.Err.	t-Value	Sig. of t
NETTOT4	-.04332	-.11919	.027	-1.606	.110
COVARIATE	Lower -95%	CL-	Upper		
NETTOT4	-.097		.010		

* * ANALYSIS OF VARIANCE -- DESIGN 1 * *

Tests of Significance for SCC_ using UNIQUE sums of squares
Source of Variation SS DF MS F Sig of F

WITHIN CELLS	1708.50	179	9.54		
REGRESSION	228.55	1	228.55	23.94	.000
CONSTANT	2005.01	1	2005.01	210.07	.000
PHASE_NO	231.98	2	115.99	12.15	.000

Regression analysis for WITHIN CELLS error term
Dependent variable .. SCC_

COVARIATE	B	Beta	Std.Err.	t-Value	Sig. of t
NETTOT4	-.12660	-.34349	.026	-4.893	.000
COVARIATE	Lower -95%	CL-	Upper		
NETTOT4	-.178		-.076		

* * ANALYSIS OF VARIANCE -- DESIGN 1 * *

Tests of Significance for DCC_ using UNIQUE sums of squares
Source of Variation SS DF MS F Sig of F

WITHIN CELLS	1584.25	179	8.85		
REGRESSION	318.04	1	318.04	35.93	.000
CONSTANT	3866.53	1	3866.53	436.87	.000
PHASE_NO	214.91	2	107.46	12.14	.000

Regression analysis for WITHIN CELLS error term
Dependent variable .. DCC_

COVARIATE	B	Beta	Std.Err.	t-Value	Sig. of t
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NETTOT4	-.14935	-.40889	.025	-5.995	.000
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COVARIATE Lower -95% CL- Upper

NETTOT4	-.199	-.100
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* * ANALYSIS OF VARIANCE -- DESIGN 1 * *

Tests of Significance for HCC_ using UNIQUE sums of squares
Source of Variation SS DF MS F Sig of F

WITHIN CELLS	1815.71	179	10.14		
REGRESSION	18.45	1	18.45	1.82	.179
CONSTANT	1142.00	1	1142.00	112.58	.000
PHASE_NO	141.10	2	70.55	6.96	.001

Regression analysis for WITHIN CELLS error term
Dependent variable .. HCC_

COVARIATE	B	Beta	Std.Err.	t-Value	Sig. of t
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NETTOT4	.03597	.10030	.027	1.349	.179
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COVARIATE Lower -95% CL- Upper

NETTOT4	-.017	.089
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* * ANALYSIS OF VARIANCE -- DESIGN 1 * *

Tests of Significance for SELF_EST using UNIQUE sums of squares

Source of Variation	SS	DF	MS	F	Sig of F
WITHIN CELLS	73159.65	179	408.71		
REGRESSION	24903.17	1	24903.17	60.93	.000
CONSTANT	147607.27	1	147607.27	361.15	.000
PHASE_NO	8295.61	2	4147.80	10.15	.000

Regression analysis for WITHIN CELLS error term
Dependent variable .. SELF_EST

COVARIATE	B	Beta	Std.Err.	t-Value	Sig. of t
NETTOT4	1.32153	.50394	.169	7.806	.000
COVARIATE Lower -95%		CL-			
NETTOT4	.987		1.656		

Table No 34 indicates that Affect Balance (nettot4) covaries across the three phases with each of the psychological variables (Crown Crisp Experimental Index Scales) except Obsessionality (OCC) and Hysteria (HCC), neither of which achieved significance in the previous multivariate analysis. Self-Esteem is also observed to covary with nettot4 across the phases. This result supports the view that Affect Balance is an important component in these psychological variables and their changes across the study period.

SOCIAL VARIABLES

The variables included in this part of the analysis are Availability of Attachments (AVAT), Adequacy of Attachments (ADAT%), Availability of Social Integration (AVSI), Emotional Support (SupP), Disturbed behaviour, (DisP), Burden experienced because of disturbed behaviour (DisB), and Practical Support (SupP).

It is noted from the table for social variables in appendix B that all the multi-variate tests are significant at phase 1 but by phase 7 only the Hotellings and Wilkes tests are significant. It would seem that by phase 7 newab4 had lost some of its power to refute the hypothesis that it is a factor in the social variables considered jointly. This is not too surprising since changes over the study period make some of these measures less appropriate for testing at phase 7.

At phase 1 all the social variables with the exception of Emotional and Practical Support are significant. That these two support variables are not significant has been noted already in another part of the data analysis. The most probable reason is that the items involved in these two measures relate to spouses and neighbours. Neighbours are probably not a targeted group as far as help seeking is concerned for this type of sample. At phase 1 Affect Balance tends to be crucially involved with an individual's close relationships and social contacts and also with problem behaviours and their effects.

By phase 7 the relationship of Affect Balance (newab4) with Availability and Adequacy of social integration ceases to remain significant. A ready explanation for this change has also been noted previously. The Affect Balance measure can be readily acknowledged as an important component in these scores at phase 1, but by phase 7 certainly, differing evaluations of the individual's view of of the adequacy of his social relationships can be expected. Higher or different

expectations are likely to be a feature of an individual's view of his social relationships. Higher Affect Balance scores have not translated into commensurate increases in social relationships as discussed before, probably because it is unrealistic to expect substantive changes in the time period sampled. Also of course, the members of the WinCode 7 group spent a good deal of time and energy attending the recovery programme.

Again it is the case that an hypothesis negating the influence of Affect Balance in relation to these social variables is not substantiated by a multivariate analysis of variance. Affect Balance on the basis of this data appears to be a substantial factor in a variety of social variables. Its presence is detected in a variable fashion across the programme recovery period.

The analysis is continued for the social variables by constructing a correlation matrix of within-individual change scores between phase one and phase seven to examine the relationship of Affect Balance (nett4) and Social Variables in a context of changing scores. The previous result indicated that nett4 was a factor present in these social variables at the three static phases. The present analysis explores whether any relationship exists in the rather different context of change, as was done for the drinking variables previously.

Table No. 35

Correlation Matrix for Affect Balance (nett4) and Social Variables

CORRELATIONS /VARIABLES AVAT TO NETTOT4

Correlations:	AVAT	ADAT_PCE	AVSI	ADSI	DISP
AVAT	1.0000	.4648**	.2204	.0743	-.2185
ADAT_PCE	.4648**	1.0000	.1083	.2008	-.4375**
AVSI	.2204	.1083	1.0000	.2114	-.2633
ADSI	.0743	.2008	.2114	1.0000	-.1733
DISP	-.2185	-.4375**	-.2633	-.1733	1.0000
DIS_B	-.2273	-.2797	-.1400	.0037	.8432**
SUPP_P	.1503	.0967	-.0834	-.1964	.0992
SUPP_R	-.0074	-.0621	.0972	-.1324	.0761
NETTOT4	.1945	.2473	.1176	.1929	-.3397*

Correlations:	DIS_B	SUPP_P	SUPP_R	NETTOT4
AVAT	-.2273	.1503	-.0074	.1945
ADAT_PCE	-.2797	.0967	-.0621	.2473
AVSI	-.1400	-.0834	.0972	.1176
ADSI	.0037	-.1964	-.1324	.1929
DISP	.8432**	.0992	.0761	-.3397*
DIS_B	1.0000	.0448	.1119	-.2207
SUPP_P	.0448	1.0000	.0100	-.0106
SUPP_R	.1119	.0100	1.0000	-.2351
NETTOT4	-.2207	-.0106	-.2351	1.0000

N of cases: 61 1-tailed Signif: * - .01 ** - .001

The correlation matrix indicates that there is only one instance where nett4 correlates with another variable. This is the Disturbed Behaviour Present Scale (DISP). This result is weaker than the previous multivariate analysis of social variables in that nett4 is much less in evidence when change scores are scrutinized - indeed it is hardly in evidence at all. These last two sets of results are not contradictory since a consideration of nett4 as a factor in other social variables at a single point in time is not the same thing as a consideration of a change score. The presence of a relationship at a single point in time does not imply

that such a relationship should exist between two or more points in time since change scores measure a process and not a static situation.

As noted previously, perhaps the time scale involved in this study is too short to effect substantial movement in the social variables such that the magnitude of the changes in them would correlate with nettot4. It was noted in comment on the table of change scores across the seven phases that most scores recorded, including nettot4, drew to a standstill around phase four before picking up again towards the end of the study period. This would seriously reduce the nettot4 change score capability of correlating with the social variables in the above correlation.

As previously noted, the design employed in this study does not easily facilitate the production of conventional outcome data. Wincode (i.e. length of time in the treatment programme) is the only usable outcome variable in the study. Sixty one of the one hundred and sixty respondents remained in the treatment until the seventh wave of data collection.

Since Affect Balance and its relationship to other study variables and outcome is a major concern of this thesis, its utility as a predictor of outcome, alongside other study variables, will be explored in the next and final analysis.

For this analysis, the total data set for phase one (wincode one) was used to predict a range of dependent variables which were nominated on a commonsense basis because they were considered to be representative of conventional

outcome criteria encountered in the Alcohol Problems treatment literature. They are drawn from the three conceptual domains of the instrumentation used in the study: Alcohol Use, Psychological and Social well-being, together with wincode. As noted above, wincode was a measure of time spent in the treatment programme; it cannot be construed as being indicative of either drinking status or general well-being.

The six dependent variables examined were as follows:

Alcohol Use

1. Drinking-related Locus of Control (DRIE)
2. Severity of Alcohol Dependence (SADQ)

Psychological state

3. Free Floating Anxiety

Social state

4. Perceived adequacy of attachments (ADAT%)
5. Social Performance (SocP)

and

6. Wincode

These dependent variables were each included in a stepwise multiple regression. In this procedure the variables were introduced one at a time in successive stages, raising the dimension of the analysis by one each time. The variables which provide the greatest reduction in unexplained variance in the dependent variable at each stage were retained in the analysis. The process continued until no further explanation of variance. Variables that had no explanatory power were dropped from the analysis and do not appear in the results table.

Six separate analyses were performed. The results

are listed in abbreviated tables (Nos. 36-42) below. The complete results of the SPSS/X print out for the analysis can be found in Appendix C.

It will be noted that the pool of independent variables from which the predictions were made varied in some of the analysis, because in some cases specific independent variables were not logically distinct from one of the dependent variables; for example, Anxiety and Depression are quite properly seen as symptoms of Alcohol Withdrawal, therefore in the prediction of Severity of Dependence, the other Crown Crisp scales have been removed from the analysis because of high inter-correlation.

The main results of the six stepwise multiple regressions were as follows:

I. Predicting Drinking Related Locus of Control

The independent variables used in this regression were:

1. General Alcoholism Score (G_Alc_Us)
2. Severity of Alcohol Dependence (SevDep)
3. Self Esteem (Self-est)
4. Availability of Attachment (AVAT)
5. Adequacy of Attachments (ADAT)
6. Adequacy of Social Integration (ADSI)
7. Availability of Social Integration (AVSI)
8. Disturbed Behaviour Present (DisP)
9. Burden felt because of disturbed behaviour (DisB)
10. Social Performance (SocP)
11. Burden felt because of inadequate soc. performance(SocB)

12. Support Received (SuppR)
13. Affect Balance (nett4)
14. Age

The results of this analysis are shown in Table 36.

Table No. 36

Stepwise Multiple Regression of Predictors of Drinking-related Locus of Control

Independent Variables	Mult-corr	Variance accounted for
Self Esteem (self-est)	.4069	.1656
Affect Balance (nett4)	.4504	.2029
Age	.4788	.2292

 Affect Balance accounted for just under 4% of the prediction of Drinking-related Locus of Control (DRIE). It was one of three variables retained by the procedure, which cumulatively accounted for 23% of the prediction. The most potent variable was self esteem, which accounted for just under 17% of the variance; the least powerful predictor was age, which added 3% to the prediction.

II. The Prediction of Alcohol Dependence (SADQ)

This regression involved the following independent variables:

1. Self Esteem (Self-est)
2. Drinking-related Locus of Control (DRIE)
3. Availability of Attachments (AVAT)
4. Adequacy of Attachments (ADAT)
5. Availability of Social Integration (AVSI)
6. Adequacy of Social Integration (ADSI)
7. Social Performance (SocP)

8. Burden felt because of inadequate social performance(SocB)
9. Support Received (SuppR)
10. Affect Balance (Nettot4)
11. Age

The results of this analysis are shown in Table 37.

Table No. 37

Stepwise multiple regression of the predictors of severity of alcohol dependence (SADQ)

Independent Variables	Mult Corr	Variance accounted for
Affect Balance(nettot4)	.3050	.0935

 In this analysis Affect Balance was the only independent variable to emerge in the prediction of the dependent variable. It accounted for 9% of the variance of the prediction.

With both the alcohol related dependent variables it was necessary to drop some variables from the pool of possible independent variables because of overlapping items. In the case of Drinking-related Locus of Control, the Crown Crisp scales (Acc-Hcc) have been left out of the analysis. In the case of Severity of Alcohol Dependence both the General Alcoholism Scale (from the Alcohol Use Inventory) and the Crown Crisp scales were excluded.

III. The prediction of Free Floating Anxiety (ACC)

The independent variables employed in this analysis were:

1. Self Esteem (Self-est)
2. Drinking-related Locus of Control (DRIE)
3. Availability of Attachment (AVAT)

4. Adequacy of Attachments (ADAT)
5. Adequacy of Social Integration (ADSI)
6. Availability of Social Integration (AVSI)
7. General Alcoholism Score (G_Alc_Us)
8. Disturbed Behaviour Present (DisP)
9. Social Performance (SocP)
10. Burden felt because of inadequate soc. performance(SocB)
11. Support Received (SuppR)
12. Phobic Anxiety (Pcc)
13. Obsessionality (Occ)
14. Somatic Concern (Scc)
15. Depression (Dcc)
16. Hysteria (Hcc)
17. Affect Balance (nett4)
18. Age

The results of this analysis are displayed in Table 38 below.

Table No. 38

Stepwise multiple regression of predictors of Free Floating Anxiety

Independent variables	Mult. Corr.	variance accounted for
Depression (Dcc)	.6685	.4468
Phobic Anxiety (Pcc)	.7105	.5047
Affect Balance(nett4)	.7442	.5539
Gen. Alcoholism (G Alc Us)	.7644	.5844
Disturbed Behaviour(DisP)	.7758	.6018
Age	.7848	.6158
Obsessionality(Occ)	.7925	.6280

In this analysis Affect Balance added 5% to the prediction of the dependent variable. In total seven variables accounted for 63% of the variance in the Free

Floating Anxiety Score. It was not surprising that Depression and Phobic Anxiety figured prominently in the results. Affect Balance came third in the table, adding an additional 3% to the prediction and was a more powerful predictor than the four remaining variables retained by the procedure.

IV. Predicting Adequacy of Attachments (ADAT%)

In this regression the following independent variables were included:

1. General Alcoholism Score (G_Alc_Us)
2. Severity of Alcohol Dependence (SadQ)
3. Drinking-related Locus of Control (DRIE)
4. Self Esteem (Self-est)
5. Adequacy of Social Integration (ADSI)
6. Availability of Social Integration (AVSI)
7. Disturbed Behaviour Present (DisP)
8. Burden felt because of Disturbed Behaviour (DisB)
9. Social Performance (SocP)
10. Burden felt because of inadequate soc. performance(SocB)
11. Support Present (SuppP)
12. Support Received (SuppR)
13. Affect Balance (Nettot4)
14. Free Floating Anxiety (Acc)
15. Phobic Anxiety (Pcc)
16. Obsessionality (Occ)
17. Somatic Concern (Scc)
18. Depression (Dcc)
19. Hysteria (Hcc)
20. Age

The results of this analysis are shown in Table 39.

Table No. 39

Stepwise multiple regression of the predictors of Adequacy of Attachments (ADAT%)

Independent variables	Mult Corr	Variance accounted for
Adequacy of Soc. Int (ADSI)	.5058	.2559
Affect Balance (Nettot4)	.5384	.2899

In this analysis the dependent variable assesses satisfaction with close relationships. Affect Balance was observed to add 3% to the prediction. The only other independent variable to contribute to the prediction in this analysis, and by far the most influential, was Adequacy of Social Integration: a measure of satisfaction with wider friends and acquaintances. This variable accounted for nearly 26% of the variance in the prediction. This is not a surprising result, given the continuity of these two variables. The two independent variables jointly account for 29% of the variance in the dependent variable.

V. Predicting Social Performance (SocP)

The following independent variables were employed in this regression:

1. General Alcoholism Score (G_Alc_Us)
2. Severity of Alcohol Dependence (SadQ)
3. Drinking-related Locus of Control (DRIE)
4. Self Esteem (Self-est)
5. Availability of Attachments (AVAT)
6. Adequacy of Attachments (ADAT)

7. Adequacy of Social Integration (ADSI)
8. Availability of Social Integration (AVSI)
9. Support Present (SuppP)
10. Support Received (SuppR)
11. Affect Balance (Nettot4)
12. Free Floating Anxiety (Acc)
13. Phobic Anxiety (Pcc)
14. Obsessionality (Occ)
15. Somatic Concern (Scc)
16. Depression (Dcc)
17. Hysteria (Hcc)
18. Age

Table 40 displays the results in this analysis.

Table No. 40

Stepwise multiple regression for the prediction of Social Performance (SocP)

Independent variables	Mult Corr	Variance accounted for
Affect Balance (nettot4)	.3307	.1093
Self Esteem (SelfEst)	.3625	.1314

Two independent variables contributed to the prediction in this analysis. Affect Balance was by far the most powerful, accounting for nearly 11% of the variance in the dependent variable. The independent variable, Self Esteem added a further 2% to the prediction. Jointly these two independent variables accounted for 13% of the variance in the dependent variable. What was being predicted here was a measure of the performance of everyday roles and obligations. Given the construction of the Affect Balance Scale with its

wide coverage of life domains, rather higher expectations of its performance in this analysis might have been entertained.

VI. Predicting Wincode

The independent variables included in this analysis were as follows:

1. General Alcoholism Score (G_Alc_Us)
2. Severity of Alcohol Dependence (SadQ)
3. Drinking-related Locus of Control (DRIE)
4. Self Esteem (Self-est)
5. Availability of Attachments (AVAT)
6. Adequacy of Attachments (ADAT)
7. Adequacy of Social Integration (ADSI)
8. Availability of Social Integration (AVSI)
9. Disturbed Behaviour (DisP)
10. Burden felt because of Disturbed Behaviour (DisB)
11. Social Performance (SocP)
12. Burden felt because of inadequate Soc. Performance (SocB)
13. Support Received (SuppR)
14. Affect Balance (Nettot4)
15. Free Floating Anxiety (Acc)
16. Phobic Anxiety (Pcc)
17. Obsessionality (Occ)
18. Somatic Concern (Scc)
19. Depression (Dcc)
20. Hysteria (Hcc)
21. Age

The results of this analysis are shown in Table 41.

Table No. 41

Stepwise multiple regression for the predictors of Wincode

Independent variables	Mult Corr	Variance accounted for
Age	.2434	.0592
Adequacy of Attach(ADAT%)	.2994	.0896

This analysis revealed that Affect Balance did not predict duration of contact with the treatment programme. Two other variables were predictive, but only explained 9% of the total variance. These two variables were age, which explained 5.99% of the variance, and Adequacy of Attachments, which explained 3% of the variance.

CONCLUSION

In this chapter Affect Balance (Nettot4 and Newab4) has been subjected to a variety of manipulations and statistical tests to observe its performance in relation to other study variables.

A table of change scores by phase provided a view of the way variables changed across the study period and their relationship to each other. Affect Balance was observed to covary with a range of other study variables, namely the psychological variables. A correlation analysis gave further indications of relationship between Affect Balance and other study variables at three phases across the study period when raw score values were used. When change scores were correlated the range of relationships for Affect Balance was greatly reduced. Possible explanations for this result were noted. The relationships for Affect Balance when raw scores

were correlated were further supported by a multivariate analysis of variance.

The data analysis reported in this chapter suggests that Affect Balance relates in an important manner to a range of other variables, particularly the psychological variables, considered to be important in studying the recovery process in alcoholism treatment.

The pattern of changes in score values over the seven phases of the study period was not one of steady progression. Progress tended to be initially quite rapid, then tailed off considerably until the fourth month, when some scores actually reversed; thereafter progress was resumed up to the end of the study period. It was noted that this overall pattern of change was not unusual in Alcoholism recovery programmes.

By the end of the study period, on most of the instrumentation employed in the study, the Wincode 7 study group tended to present profiles which were either the same as, or were moving close to, what might be considered normal for the general population. When the Drinking, Psychological and Social data sets are considered at the end of the study period the impression is one of considerable change from the state of affairs that was observed at the beginning of treatment.

This chapter has tried to explore the status of the Affect Balance measure as a factor in these improvements. It was observed to be an important factor in the improvement charted for the Wincode 7 study group.

The study design was not appropriate for testing

direct casual relationships between Affect Balance and other study variables, although it was established to an unknown degree that Affect Balance was a factor in the psychological and social variables and as such is worthy of further explorations in a design where an experimental approach can be employed to try and tease out causal relationships. In terms of Tuckfeld's model discussed previously, Affect Balance might be seen as important for the resignation period where disengagement and commitment to further action are the key factors through its relationship with psychological factors; and in addition for the termination phase with its focus on life-style adjustment, by virtue of Affect Balance's impact on social variables.

Overall the stepwise multiple regression analyses indicated that Affect Balance was only predictive to a moderate degree (3-11%) of the five dependent variables related to the initial wave of data collection (wincodel). Moreover Affect Balance scores at initial data collection did not emerge as a predictor of the length of time the 166 respondents spent in contact with the treatment programme. The main predictor of amount of treatment received was the respondent's age.

CHAPTER 7

EMOTION, AFFECT BALANCE AND ALCOHOLISM

As noted in previous chapters Affect Balance, to be more than merely a manipulation of summary statements about life-circumstances, or a judgement of happiness or well-being, requires to be predicated in a conception of emotion. This is necessary to give substance to what was discussed briefly as the second aspect of Affect Balance. This chapter explores this issue and attempts to relate these matters to an understanding of social science research including alcoholism and its treatment. I want briefly to consider the development of ideas concerning what emotion is, that have developed from an analysis of the emotions from early times to the present. This seems proper since, in fact, some of the early views on emotion provide a bedrock for modern theory of emotion.

For a wide-ranging review of this field from a theoretical perspective see Lyons (1981) and from a predominantly psychological-empiricist viewpoint see Frijda (1986); for an historical account see Berrios (1985).

The first difficulty encountered in the field of emotion studies, is one of semantic confusion. Berrios has this to say about the terminology involved, "Mood, affect, sentiment, emotion, passion, agitation and propensity (inter alia), constitute a family of terms with a protean referent. They have varied etymological origin, and stem from different historical epochs". He suggests that sentiment, emotion and passion have been distinguished from mood, affect and feeling by a variety of criteria such as duration, polarity, intensity; and by being

associated with an inner or an outer object, amongst others. Mood and affect are regarded as having a "reasonable" duration, and being without immediate objects such that they provide "a sort of background feeling tone to the individual". Sentiment, emotions and passions are defined as being feeling states, "that are short lived and more or less intense, salient and related to a recognizable object". Passion is conceived of as an intense form of emotion with which bodily changes are associated. On this reading of the semiotics involved, mood is the background feeling tone upon which emotions, which are either congruent (synthymic) or incongruent (catathymic), can be superimposed. Affect is said additionally to be dispositional in nature (Ryle 1949); this accords well with the notion of Affect Balance. In the literature, all these terms are usually subsumed under the single term, emotion.

A variety of theoretical perspectives of emotion exist, some overlap, still others provide competing theses. It is probably the case that the variety of theses about emotion are more a reflection of the researcher's temperaments than anything else; a special case perhaps of theory-dependent observations, the theory in question being the writers' philosophical views about the nature of man and his relationship to the external world.

"What is an emotion?" This was the question asked in the title of a paper by William James published in the journal "Mind" in 1884 (James 1884). At the time that this paper was published, towards the end of the 19th century, James held the post of Professor of Philosophy at Harvard University, having

switched from teaching in the medical faculty at that university. He was to publish his famous and influential book, "Principles of Psychology" in 1890 (with a section on emotion), that marked him off as one of the founding fathers of the then new discipline of Psychology.

James' answer to his question was that emotion is a consequence of the physiological changes consequent upon awareness of events occurring in the external environment. A man might perceive a threat, such as a speeding car travelling towards him out of control; characteristically, the man would experience a tightening of the muscles and generally become aroused to a state of preparedness, ready to take flight from the impending danger, the visceral changes taking place within him being experienced as "an emotion, or emotions".

At the same time that James was developing his theory of emotion, a Danish psychologist, G. Lange was working along similar lines. For Lange emotion was the awareness of circulatory changes as opposed to the visceral changes of James' theory. In 1885 James and Lange jointly authored a book called simply 'Emotions' (James and Lange 1885). The James-Lange theory of emotions, as it came to be known, dominated the field of study of emotion until it was fatally attacked by Walter Cannon in the late 1920s and early 1930s (Cannon 1927; 1931). However this theory exerted a great deal of influence in Philosophy and in Psychology, even after Cannon had pointed out its inability to distinguish among similar emotions and its failure to explain the mentalistic and behavioural aspects of emotion. Cannon in many senses laid the James-Lange theory to rest, but it

nevertheless tends to be regarded as the beginning of a modern scientific study of emotion, its influence continuing well into recent times in respect of its emphasis upon a physiological explanation of emotion. For instance the assumption that the behaviour was the emotion was very appealing to Behaviourists long after the refutation by Cannon of the James-Lange conceptions.

James, as a professional philosopher, was intimately involved with philosophical debates concerning Pragmatism, but equally as a philosopher, he was well aware of the long philosophical tradition of work on emotion; particularly its beginnings with Plato and Aristotle. Between the debates of these two Greek philosophers over the nature of emotion, and the famous James-Lange theory, there is an expanse of time which has been populated with theories of emotion of one type or another which extends up to the present day. In a sense James established the final pole on a continuum which had been in the process of formation for over 2,000 years. Interestingly Aristotle's descriptions of emotion are very close to much modern work in this field.

Plato (1970) made a clear distinction between the rational and the irrational parts of the soul. The passions (impulses and desires) can be within the control of reason. The soul (psyche) embodied both of these elements; the rational part was immortal and the irrational part, mortal. They were, according to Plato, located in the head, and trunk of the body respectively. The rational psyche (soul) is essentially independent of bodily processes, but this is not so in the case

of the mortal psyche and its passions. These are intimately connected with bodily functions. The passions then, according to this account, are located in the baser, mortal parts of the soul. They were inferior to, and in control of, the immortal, rational aspects of the psyche. This manner of distinction, originating in the ancient Greek world, has been of momentous consequence for Western culture generally, right down to the present time. Berrios (1985, 1984) discusses how the inferior role allocated to emotion in the 3rd. century B.C. has had an important inhibiting influence upon recent psychiatric views concerning affective psychopathology and descriptive psychopathology generally. This relegation of emotion to an inferior status to the intellect is largely responsible for the neglect of research on emotion.

Plato's pupil, Aristotle, provided an account of emotion (Aristotle, 1941) which had a profound effect upon later theories of emotion, and is particularly important for modern cognitive theories (Arnald, 1960). Aristotle was concerned with distinguishing between action and passion. This interest centred around a view that orators could perhaps exert an influence on their audience through affecting their passions. His theory is a cognitive account since, for him, cognitive judgements were central to emotion. To study an emotion, Aristotle argued that three distinct pieces of information were required. A description of an individual's state of mind, what the emotion was directed at, and, finally, the grounds upon which the emotion was generated. All three pieces of information were necessary, Aristotle claimed, in order to make

sense of an emotion. Aristotle held the view that emotional states, feelings, impulses etc., are aroused by our judgements concerning events and things; what we think about the world. Belief is the major determinant of emotions according to Aristotle, that is to say, the belief in the necessary cause of feeling and the consequent physiological aspects of emotion. In general terms he sees human beings as having a strong moral belief about how others should behave.

In his discussion of Aristotle's theory, Lyons (1980) points to the phrase "state of mind" as being too vague and that the belief aspect of the state of mind needs to be separated in analysis both from the evaluative and the appetitive (wants) aspects. Lyons argues that the emphasis needs to be placed upon the evaluative rather than on the belief or the appetitive aspects. The reason being that different emotions can arise from the same beliefs.

Aristotle's account of emotion avoids the mind-body dualism and Plato's (as well as many later writers') sharp distinction between the rational (cognitive), and irrational (physical) elements of emotion. For him emotions are not irrational uncontrolled responses to life's vagaries. His complex cognitive theory of emotion has just recently been rediscovered by philosophers and others working on emotion-theory. Aristotle, in many crucial aspects, rather than James, can be considered more modernistic and relevant to current debates in emotion theory. In view of this it is probably better to consider emotion theory in terms of schools of thought rather than chronologically.

Lyons, in his book, discusses four "classical theories of emotion and a fifth which he espouses. They are, 1. Feeling Theory, 2. Behaviourist Theory, 3. Psycho-analytic Theory, 4. Cognitive Theory; and 5. The Causal-evaluative Theory. Colhoun and Solomon (1983) organize their book on Emotion around "five models of emotion". The models are discussed in terms of, 1. Sensation and Physiological theories, 2. Behavioural theories and 3. Cognitive theories. I shall briefly consider Calhoun and Solomon's three global theories.

SENSATION AND PHYSIOLOGICAL THEORY

Such theories stress the "feel" of emotions. A feeling inside the organism reflected in popular idioms such as "I feel down to-day", or "the pangs of remorse", the "sinking feeling of fear" and so on. What is being identified here are everyday feelings of excitement and arousal etc. which accompany emotive experiences. Emotion is a feeling which occurs to the person. It may occur in the head, the heart or the stomach etc. depending upon the particular emotion. The experience of emotion is the same for sensation and physiological theories. The distinction between the two is made in the focus of analysis. An example of sensation theory was provided by David Hume in his "Treatise on Human Nature" written in 1739, and which he described as "falling stillborn from the press". It is of course now considered to be an important book (Hume 1955). For Hume, physiological aspects of emotions were unimportant, the characteristic feel of the emotion was what was crucial. Fine distinctions can be made among the components of mild emotions that do not have vigorous bodily accompaniments. Hume

was so sceptical about the sources of knowledge that were available to individuals that the feel of things like the rightness or wrongness of an action, love, appreciation and so on became the important issues for him.

Physiological theory counts the physical disturbances as being important, even if the awareness of such internal agitation is barely perceptible, as might be the case with very mild emotions. William James can be taken as being representative of this school of thought.

Both these approaches to emotion have to make use of causal analysis which repudiates cognitive elements that are constructed out of decisions, beliefs etc.; the unit of analysis is a basic feeling that is unitary. Descartes provides the most mechanistic analysis available in the literature, particularly his analysis of fear in the "Meditations" (Descartes 1961).

BEHAVIOURAL THEORY

These theories concentrate upon observable behaviour when analysing emotion, a very different approach from the subjective "feel" of emotion. For the most part, feelings are considered inadmissible by behavioural theorists. For this school of thought, it is not the internal feeling which is analysed but rather the same experience that is observed by an outsider; as when we say such-and-such a person was "flushed with embarrassment", "disgust", "anger" etc. They claim private experience leads, in analysis, to paradoxical outcomes. How can we have knowledge of others' private experience? The first Behaviourist theories of emotion were produced at a time when psychology was emancipating itself from philosophy, and aligning

itself with Natural Science. Hence old methods like introspection had to go and only the methods of Natural Science were permissible. This of course was the position in Behaviourism generally as well as behaviourist work done on emotion.

Watson, generally agreed to have been the founder of Behaviourism, was interested in the study of emotion. He attempted (by consensus, unsuccessfully) to show that emotion was a behavioural pattern that was inherited. He tried to demonstrate this by somewhat crude and cruel experiments with babies; frightening them for example, and observing their behaviour (Watson 1919).

Although one can hardly describe him as a behaviourist, Charles Darwin was influential in emotion theory by his Behaviourist-like observations which he had published in his book, "The expression of Emotion in Man and Animals". He conceived of emotions as internal events which could be observed and interpreted according to three principles. Serviceable habits. i.e. relieve sensations and gratify desires. The opposite of this was the principle of Antithesis, e.g. rage etc., and the principle of Direct Action, physiological changes that prepare the organism for action (Darwin 1965). Dewey developed an account of emotion out of a criticism of Darwin. Dewey argued that the object of emotions and the peculiar "feel" of an emotion both result from emotional behaviour. Dewey also attacked James' theory of emotions in his paper "The Theory of Emotion" published in 1894. Essentially, he produced a three-factor theory. 1. "Quale", or feel. 2. Purposive behaviour.

3. An object that embodies emotional quality (Dewey 1894). B. F. Skinner gives a simple stimulus-response account of emotion (Skinner 1974, Skinner and Holland 1961). All psychological behaviourists like Skinner, and philosophical behaviourists like Ryle (1949), argue that all mental terms can be reformulated in behavioural terms.

EVALUATIVE THEORIES

There is a wide variety of such theories but all see emotion as having a rational basis: emotions are important mental phenomena. Some theorists like Sartre (1962) and Solomon (1973), view emotions as resembling value-judgements or beliefs. Fear, in this view, is akin to the belief that a threat is present, happiness a belief that all is well. Others of this genre see emotions as analogous to sensory perceptions. Max Scheler (1970) represents this view, namely that emotion or feeling is a form of thinking that enables an individual to derive values from an environment of facts. Colhoun and Solomon (1984) summarize this view by quoting Blaise Pascal's aphorism "The heart hath its reasons which reason does not know". Emotions are a sort of insight, for Scheler (1970).

Brentano deals with emotions as part of a project to unify psychology. He insists that emotions, like all mental phenomena, are directed toward some manner of object. The object may be of the imagination, like pink elephants, or immaterial symbols. This principle of "Intentionality" is central. Brentano's perspective represents a rejection of emotions as mere sensations. We can experience positive or negative feelings about an object; this is emotion as a pro or

con feeling. Brentano puts special emphasis upon love and hate as poles of the range of evaluation of an emotion. By choosing the right pole for our emotions, we can gain insights about the object of an emotion (Brentano 1971). Brentano and Scheler are within a tradition of moral sentiment theorists who studied emotion as part of a wider project concerned with the validity of value-knowledge. In some respects this is very different from the type of theory proposed by Sartre and other evaluative theorists, where values are projected onto objects instead of being derived from them.

COGNITIVE THEORIES

In these, emotions are considered to be either cognitions or logically dependent on cognitions or beliefs about states of affairs. I have discussed Aristotle, who is representative of this school of thought. Modern theorists of this school tend to be influenced by linguistic philosophy which insists that, to give an account of emotion we must pay attention to the way emotion statements are used, using logical restrictions or linguistic conventions or rules, as guidelines. Errol Bedford is a prominent exponent of this line of thinking. Bedford argues that emotions logically entail both evaluative and factual beliefs, and that different emotions have sets of beliefs appropriate to them. Bedford makes a distinction common to this school, that emotions and feelings are separate. Emotions may be unreasonable or inappropriate but this cannot logically be said of feelings. Therefore, the two necessarily are distinct (Bedford 1962).

R.S. Peters, another member of this "school", highlights

the causal role of appraisals in emotions. We can not know what sort of emotions another individual is experiencing without having some kind of knowledge about their appraisals of situations. Peters sees emotions as being passively received as opposed to motives which connect appraisals to things acted out (Peters 1970).

These then are the sorts of core-theories of emotion which have provided justification for much empirical work in the field. Much recent psychological and sociological work on emotions draws upon these basic ideas. As an illustration: the extremely heated debate between Zajonc and Lazarus conducted through the medium of the American Psychologist in 1984 over the role of cognition in emotion. Zajonc (1984) takes a sensationalist stance, and Lazarus (1984) an evaluative position. The Cognitionist Peters, would have said, had he been involved, that it all depends upon what they wanted to count as emotion. As the debate developed, it became apparent that they were not arguing with shared definitions of what counted as emotion(s). To some extent, this polemic polarized the field of emotional studies. This very recent and high profile argument is cited to underscore the value of the types of basic theories discussed thus far.

The task in hand now, having briefly considered the range of core theories of emotions, or at least a representative slice of them, is to determine what aspects of these theories will help make sense of Affect Balance. The last class of theory, that concerning a cognitive approach, is helpful, but only in a peripheral sense. They certainly assist in the

avoidance of illogical statements and there is plenty of scope for this as I elucidate Affect Balance. Cognitive theory can be seen as an analytic tool useful in a methodological approach to Affect Balance.

The essence of Affect Balance and its value for the type of research reported in this thesis would clearly best be served by being grounded in an Evaluative theory of emotion. The very heart of the concept of Affect Balance is its ability to evaluate or judge the pleasantness or unpleasantness of various life circumstances. The emotions here have very specific objects in view: life-domains. The principle of Intentionality certainly is of great relevance in this respect. Robert Solomon points out that the cause of emotion is not the same thing as the object of the emotion. This is helpful when considering life-domains either singly or in aggregate. A state of affairs does not necessarily influence our emotional response, although it might be a contributory factor.

An example from the present research might be unemployment. Two unemployed individuals might feel extremely distressed due to their lack of employment and each to the same extent, despite the fact that one of them might have been responsible for their unemployment, whilst the other one was not.

Theoretical explanations also appear to be available for what was described as the second aspect of Affect Balance as well as its surface mechanism. No particular name was given to this facet of the concept because no convenient label in the appropriate psychological literature can be found. However, it corresponds in some seminal respects with those emotion

theorists who see an individual's emotions as active things which colour their world.

The type of emotion theories discussed so far are essentially meta-theories that have provided the conceptual undergrowth from which much modern work on emotion has evolved. It now becomes necessary to explore the link with emotion at the research or empirical level.

Affect Balance as Empirical Tool

After a considerable period of being ignored by researchers in Psychology and Sociology, emotion is once again a fashionable area of research. Research and theorizing about emotion over the last two decades has considerably increased, many new theoretical approaches of a more focused type than the classical theories discussed above have been published. Plutchik (1980) counted 28 different definitions of emotion and he thought the list could easily be extended. Philip Shaver began his editor's introduction to the Review of Personality and Social Psychology 5, given over to papers on emotion in 1984, by saying "Psychologists are currently returning in droves to the study of emotions, an obviously important topic that was swept off centre stage several decades ago, thanks to Behaviourism, and then ignored a while longer when Cognitivism captured the limelight" (Shaver 1984).

Having located Affect Balance in a core-theory of emotion of the evaluative type, it should be noted that this type of theory is more often than not referred to as a cognitive theory in the modern cognitive psychological literature. It will be helpful to see where a concept of Affect Balance might

cohere with ongoing research. Two of the most important characteristics of the Affect Balance processes are, the dimensions of pleasure-displeasure used for the evaluation of life-domains, and the person-situation interaction inherent in this judgement. A related issue is the higher order notion of hedonic tone.

Validity studies of the Delighted-Terrible scale were conducted by Andrews and Withey (1976). [See also Andrews (1974), Andrews and Withey (1974) and Andrews and Crandall (1976)] These studies however, relate to happiness and well-being. A link must be made between Affect Balance scores, both domain and balance score, with a control measure of emotion. There have been many studies of the dimensionality of emotion "Appraisals of each ongoing and changing transaction with the environment with respect to its significance for an individuals, well-being are ubiquitous within the species" (Lazarus et. al. 1984). A crucial aspect of appraisal for the Affect Balance measure and its underlying model, are the dimensions of pleasure-displeasure which, in the instrument are presented as the Delighted-Terrible scale for respondents.

In discussing emotion as a multi-component process, Scherer reports that a study of the dimensional approaches to emotion indicates that the three most characteristic factors of the organism's processing of antecedent stimulus-events that jointly determine the nature or type of emotional reaction are, the positive or negative dimension, which is seen to result from the intrinsic pleasantness/unpleasantness of a stimulus; the activity dimension resulting from a mismatch between an

individual's plans/goals, related expectations, and the actual state of affairs encountered; and the individual's estimate of how well they will be able to cope with the particular stimulus event (Scherer 1984,1982). An early demonstration of a pleasant-unpleasant dimension of emotion was presented by Block in 1957. He used the semantic differential method with a sample that considered emotion words. Most of the variance of the sample ratings of the words was accounted for by the factors pleasant-unpleasant and activation (Block 1957). This experiment was replicated by Plutchik (1980) and by Russell (1980) who both used a card-sorting method. Apart from having commonsense validity, the Affect Balance instrument with its delighted-terrible scale, and its conceptual validity both as a measure of happiness and as a pleasant-unpleasant scale, tap fundamental dimensions of emotion which have empirical support of a substantive sort.

Another issue related to the fundamental dimension of affect, is the higher order concept of hedonic tone. The Affect Balance score might be said to have a hedonic tone which refers to the entire range of domains. It is certainly inferred in many of the theories of emotion. Hedonic tone or hedonic level as it is sometimes called, although having relationships with satisfaction, is not the same thing. Satisfactions are tied into expectations and standards of comparison, i.e. they have a balance sheet aspect to them. Hedonic tone is a different sort of entity: it is an experience - a feeling state which is not necessarily tied to a cognitive interaction with the external world, although it usually would be. Another way of thinking

about hedonic tone is to see it as the quality of a person's affect; this would be modal hedonic tone, or the quality of a person's current affect would refer to his/her average hedonic level. It will be evident that this sort of analysis will become impossible because of our inability to engage with another person's private experiencing. This is a basic issue for the Cognitive theorist of emotion described above. There are logical impediments to any measure of hedonic tone except perhaps at very extreme degrees of high or low magnitudes (Brenner 1975).

This presents a problem for the concept of Affect Balance, or at least for what I have referred to as the second aspect of Affect Balance, because what is envisaged is a type of second order effect for which some sort of notion like hedonic tone acts like a unifying concept. Young (1961) has argued that despite the inherent difficulties with this idea, it does seem to be implicated in the making of judgements or rating of preferences. It is simply the case that hedonic tone is not amenable to measurement by organic sensation or cognitive assessments, or combinations of both (Frijda 1986). Despite its intuitive utility, hedonic tone is incommensurate with the use of reductive methods. It would appear that the closest one can get to a concept like this, is by the use of metaphor as an approximation.

The other crucial characteristic of the Affect Balance measure and model referred to above was its focus on person-situation interaction; interaction in the sense of evaluation. The Affect Balance measure that I have presented in this thesis,

like Bradburn's original scale, can be conceived of as a measure of person-situation interaction of a global sort but, like a social indicator, it can be disaggregated and limited in scope (this would of course increase the assumed error component in the statistical sense of "error").

All modern theorists are agreed that the actual environment needs to be defined in terms of the individual's perceptions of it (Pervin 1968, Stern 1970). Both Lewin (1951) and Murray (1951) debated the issue and argued that, "The same units or commensurate dimensions" should be used to assess the individual and the environment (Walsh 1973), a problem commensurate with that of hedonic tone. The Affect Balance methodology presents one sort of response to this difficulty. However, the research problems have been conceptualized in a more expansive fashion than they were in the 1930s. Indeed, the interactional aspects of the evaluations of life-domains conform to two sorts of demands that have been to the forefront of debate in personality and social psychology, one originating with Allport (1960) for adoption of idiographic rather than nomothetic methods for the study of persons. This was related to a view he had concerning the philosophical problem science has about whether scientific knowledge is knowledge of particulars or of universals (it has been noted previously that Affect Balance, as an overarching concept, might be of value in this regard). The other issue is that of the "Interactional" perspective. This has been described as the "Zeitgeist" in personality research and theory (Pervin 1978).

Pervin (1978) discusses a theoretical model of person-

situation interaction and notes that affect as a factor in such models has received scant attention particularly by personality theorists and suggests that it needs to be included. The interactional aspects of the Affect Balance measure and model appear to meet both practical and theoretical demands currently being made by researchers. Some of the theoretical and methodological detail has been noted. The Affect Balance Model is seen as going some distance towards meeting the needs of a modern person-environment fit theory, in that both the need-supplies and abilities-demand fit, can be accounted for using affect as an evaluative matrix. Recent views on this issue are discussed by Caplan (1983).

Thus far Affect Balance has been examined in relation to theoretical and research demands in a sense to test its stamina. I will now briefly consider its utility as a research tool. The literature on life events is both topical and relevant to the subject of this thesis. The problems in life event research appear very similar to difficulties mentioned above. Generally, it is felt by this writer that the stressful life event research has not achieved the promise that researchers in this field had hoped and it is suggested that an Affect Balance model might be a useful analytic tool for researchers engaged in this type of work. Kasl (1983) made the interesting observation that in comparing the two very influential books edited by the Dohrenwends on stressful life events (Dohrenwend and Dohrenwend 1974, 1981) published only seven years apart, there was a marked difference of focus. The 1974 volume had a large selection of reports of empirical

findings, and a small section on methodology. The most recent volume is overwhelmingly methodological with only a secondary emphasis on results. He notes the reversal in the usual manner of scientific progress. Kasl implies that substantive results are not forthcoming due to methodological flaws. However, he further argues that the emphasis on measurement rather than causal-aetiological problems is like "looking for keys under a street light, where one can see better, rather than in the dark alley where the keys were lost". Mechanic (1974) has noted that stressful life events "play some role in the occurrence of illness in populations", but any statement beyond this vague generalization is likely to stir controversy. This assessment is still likely to be apposite at the present time. The problems seems to be that the life event inventories do not take account of the wealth of interactions that occur both across and between aspects of an individual's daily life. It seems only to be marginally fruitful to extract discrete events from the tableau of daily life.

Certainly, alcoholics have been found to report high frequencies of stressful life events. Masuda and Holmes (1978). O'Doherty and Davies (1987) published a review of life events studies conducted with drug users, drinkers and smokers. It was found that non-relapsers had high positive events scores and that relapsers had experienced about twice as many negative events. However, they are critical of the life event approach and reiterate Mechanics' criticism noted earlier. O'Doherty and Davies say that their findings "provide evidence for some version of a life event theory". They conclude that life event

research in the area of addiction, and generally, has been "disappointing and no strong model has emerged; this despite the obvious point that a connection between environment and behaviour is a corner-stone of social-behavioural psychology". Both Kasl, and O'Doherty and Davies pin-point the problems inherent in attempting to extract discrete events from an individual's life course. Such events are intimately involved with the person's life-style. Lazarus has argued the case for continuity of appraisal in both life events and emotion research. Overwhelming evidence is to hand that individuals extract meaning from their daily life events, life course and any research paradigm in this area should take account of this (Lazarus and Folkman (1984)).

It is argued here that life event research would greatly benefit from a move away from the study of discrete events to that of life domains, perhaps with event inventories being employed as an adjunct to the design of such studies. Affect Balance, it is argued, meets the theoretical imperative to account for the individual's appraisal of the complexity of their daily life. These issues of course bear centrally upon the major problems of social science research which were noted in the chapter where Affect Balance was introduced.

Before leaving the area of life events, coping and stress research, it is worth noting that Eysenck (1983) has proposed a Stress - Personality paradigm which takes account of individual differences. Individuals with high, as opposed to low neuroticism are said to live a more stressful life, not because they encounter more stressful events, but rather because

the events that they do encounter produce more strain in them. This type of explanation it is argued, helps better explain the distribution of stress related illnesses, and others, like cancer, which have not usually been construed in this way. The relationship is said to be reversed for certain forms of illness usually correlated with high extraversion scores - type "A" behaviour - like cardiovascular disorders. To explain the high correlation between neuroticism, and the absence of malignant tumours, Eysenck proposes the notion of a stress-personality theory with an "inoculation effect". Such an inoculation effect is very close to what is being hypothesized for the psychological asset that is presumed to accrue to the person with a positive Affect Balance score calibrated for increasing degrees of hedonic tone.

Turning now briefly to matters of more direct relationship with the field of alcoholism and its treatment, the whole area of quality of life research has crucial importance for alcoholism treatment but since it has been touched upon above, and in a previous chapter no more need be said about it here. The National Institute on Drug Abuse produced a research monograph in 1980 called "Theories of Drug Abuse: Selected Contemporary Perspectives". It dealt with theories of drug and alcohol abuse - addiction, citing the views of individual theorists who had published accounts of addiction. The citations ran to just under one thousand. This is an example of the diversity of thinking in the field of addiction generally (Lettieri, Sayers and Pearson 1980). This is reason enough for not attempting to match Affect Balance with theories

of alcoholism. However, given what has been said above about the cognitive link between person-situation events and the importance of appraisal for theories of emotion, it seems propitious to take a brief look at the value-expectancy theory of alcoholism.

Hays (1985) reviews several constructs; the Internal-External Locus of Control construct, Health-Beliefs model, the Theory of Reasoned Action, Differential Association-Reinforcement Theory, and Problem Behaviour Theory. Hays suggests that all these theoretical perspectives stem from Value-Expectancy Theory, "the various forms of value expectancy have emerged as meta-theories of the original theory". He integrates Value Expectancy Theory with Social Learning Theory to provide a more complete account of alcoholism. What is of interest here is the pervasive influence of value expectation, the values being individualized values of life-goals and expectations and the probabilities of attaining these goals. Both the value placed upon a goal and the expectation of achieving it, shape the motivational force directing behaviour toward such goals. It is clear that Affect Balance methodology is quite suitable for meeting the total requirements of Value-Expectancy Theory and all the theoretical sub-sets that it subsumes. The claim made here is that Affect Balance methods are ideal for a large area of theory in the alcoholism field by virtue of their domain-specific evaluations and the nature of such evaluations as discussed previously.

The most direct link between alcoholism and emotion is of course that alcohol is perceived to have emotion-altering

effects. That is, alcoholism and heavy drinking as characteristics of an individual's life style are assumed to be related to the experience of negative affect as in the model of Affect Balance. Braucht et. al. (1973) reviewed the literature on adolescent drinking and found support for the above assumption. Problem drinking was associated with low self-esteem, anxiety, depression and the non-achievement of life-goals. Russell and Mehrabian (1975) reviewed the literature on the role of emotion and alcohol use, they concluded that emotion was implicated in every stage of drinking, before, during, and after, and that alcohol is sought at least partly for its emotion-altering effects. Emotion then, is implicated in an important way in problem drinking.

The only instance, to this writer's knowledge, of the use of an overall level of happiness scale in the alcohol literature was in Brenner (1967). The scale in question was taken from Bradburn and Caplovitz's "Happiness Study". Brenner obtained survey data on 1,515 respondents who had been asked questions about satisfaction with life and happiness. The group who reported generally getting what they wanted out of life, tended never to have an alcohol problem and were moderate drinkers when they drank. The other group, who were those who drank moderate to large amounts of alcohol per session and had encountered problems due to drinking, including those who were ex-drinkers, were appreciably less happy. Nearly all the elements of an Affect Balance model are included in this study which was undertaken before Bradburn had derived his notion of Affect Balance from the Happiness Study referenced.

All these instances which bear upon the utility of Affect Balance can be construed as instances of convergent validity of either the measure or the model of Affect Balance, or both.

To conclude with a comment upon the use of the Affect Balance measure in evaluations presented in the preceding chapters, and its usefulness for programme planning: it is clear from the results presented in this thesis, and much of the research noted above, that a desirable Affect Balance contributes, perhaps in a substantial way, to a positive outcome of treatment. The instrument used to obtain life domain assessments enables one to evaluate in a quite specific way, areas of life functioning which are frustrating to the individual concerned. This enables treatment programmes to engage in detailed treatment planning to help boost the well-being of clients. A fortunate aspect of this model is that it does not matter much which domains are targeted because there is a cumulative effect in operation. This means that those domains most amenable to change can be focused on first, and if successfully treated the incremental increase in Affect Balance will fuel still further attempts in other domains, by virtue of the presumed psychological robustness which accrues to the person with increased assets of an enhanced Affect Balance. At the same time the individual is less vulnerable to insults from the environment because of this general increase in well-being, much like Eysenck's "Inoculation Effect".

As noted previously, the experience of using the instrument with clinic attenders led to more fruitful

assessments of individual difficulties such that more cogent therapeutic agendas resulted. From a programmatic stance, this work with Affect Balance has been a potent force making for the extension and elaboration of a range of methods aimed at helping individuals to increase their social functioning.

CONCLUSION

This chapter began with a brief listing of a selection of source theories of emotion.¹ The aim was to give the concept of Affect Balance a grounding in a theory of emotion. This was seen as an essential step before any further conceptual work could proceed. This was accomplished and some of the details of the model were discussed in the context of more modern theorizing and empirical work on emotion. The model received vindication in this way, that is, all its essential components were found to have both theoretical and empirical support.

The one difficulty encountered was with the concept of hedonic tone. It was seen that this is a problem which has been considered by other theorists, and also one for which it is not possible to provide empirical support because of logical constraints inherent in the concept. Having established this sort of validity for the Affect Balance model and its attributes, its utility for a range of current theoretical and practical work was considered. It was argued that both the measure and the balance model held a good deal of promise for the research programmes discussed. In this connection it was noted that Affect Balance bears very centrally upon some of the core philosophical and methodological concerns of social science. This was the overarching concept aspect of Affect

Balance.

Lastly, its relevance for the study of alcohol problems and treatment was noted, as was its actual use and influence on the treatment programme studied in this thesis.

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CHAPTER 8

CONCLUSIONS

The present study attempted to demonstrate the value of an Affect Balance measure and its underlying model in the context of an Alcoholism Treatment evaluation.

The thesis began with a literature review which yielded evidence that progress had been made over the last few decades, but "not nearly as much as one would like"; certainly not enough for treatment programmes to invoke relevant assessment variables and correctly match clients to appropriate treatments, such that significant improvements in treatment outcomes can be achieved. It was suggested that one strategy to invoke when progress falls short of what is expected is to go back to the source model, critically appraise it and change it if necessary. To this end Affect Balance both as a treatment model and an assessment model was tested out within the constraints of the design, in the evaluation. Affect Balance was found to be a clinically relevant variable and a theoretically relevant concept.

The data analysis failed to derive a discrete profile for those who complete the study period which differentiated them from the rest of the sample at the beginning of treatment. Minor differences were observed, one of which was that the completers reported more instances of drinking than did the rest of the sample. It was suggested that this might be indicative of a more serious-minded approach to the recovery programme. Change scores for the group of completers were observed across the study period and correlations noted.

A positive trend was in evidence in most variables across the study period. The 61 subjects who remained in treatment throughout the study period (36.7% of the total sample), presented a near problem free profile in terms of population norms (or commonsense judgements in the absence of such norms). As such the treatment programme compares favourably with similar treatment endeavours reported in the treatment evaluation literature. As noted elsewhere this outcome is yet another instance of the operation of the 'rule of one third' reported so ubiquitously in the literature.

The position of Affect Balance as a key variable in both process and outcome in this study is less than clear. It was observed to correlate with a variety of other study variables, and the multivariate analysis indicated that it was a factor in a range of psychological and social variables across the study period. The analysis of covariance added weight to the multivariate analysis by showing that Affect Balance was a covariate of the psychological variables. However other analyses performed weaken somewhat its status as a new and important measure.

In the correlation of within-subject change scores, Affect Balance did not emerge as a correlate of either the alcohol related variables or the social variables. Such a result might have been expected at least in the case of the social variables. The only within-subject change score to correlate with Affect Balance was 'Disturbed Behaviour Present'.

The stepwise multiple regression showed that Affect

Balance did predict the 5 nominated outcome variables, but only accounted for fairly moderate amounts of variance, and it did not predict a sixth variable, wincode (a measure of the amount of treatment received) at all.

Affect Balance was then subjected to a variety of statistical manipulations to assess its performance as a research and clinical tool. Whilst it was frequently present and prominent in the results of various analyses, its influence was weak. This raises issues about the manner in which the concept of Affect Balance was operationalised in the present study. The measure employed in the study encompasses the possibility of rank ordering and weighing the life domains. It may be the case that a reduced number of domains weighted for importance would produce more substantial results. Probably the fairest judgement of the Affect Balance variable as employed in this study, is that the evidence for its value as a key variable is equivocal.

A range of source theories of emotions was considered with a view to grounding Affect Balance in a theory of emotion. Affect Balance, it was argued, constitutes a model that helps researchers and clinicians achieve a number of useful outcomes.

At the level of programme organisation an Affect Balance model helps with the interpretation of data that facilitates the planning of therapeutic programmes.

At the individual level Affect Balance interprets certain fundamental aspects of the life situation of clients. It does this because it is a normative concept; a value

judgement about what a humanly desirable state of affairs is. A range of hypotheses from emotion theory appeared to ground the notion of Affect Balance satisfactorily. Because Affect Balance was found to relate to a range of treatment variables it is hypothesised that an individual's balance score will enable a statement to be made about that individual as being psychologically healthy/unhealthy; enjoying a desirable or undesirable state of well-being. There is a correlation between Affect Balance and indexes of client behaviour.

Part of the rationale of this research was to introduce the concept of Affect Balance as a clinically and theoretically relevant notion to the field of alcohol studies and social psychology. It was borrowed from Quality of Life research, where it has been employed in large scale survey work. In this thesis it has been operationalized in a form suitable for research on treatment and person-situation interactions, and its theoretical boundaries were explored. It was conceived of as an overarching concept and as such was thought to bear on crucial problems in social science research. Some possible uses for it in social science research were discussed, both methodological and theoretical.

LIMITATIONS OF RESEARCH

The design of the research had practical constraints which severely limited the dual aims of the evaluation as noted in Chapter 3. Practical problems arose with some of the instrumentation, particularly the life event inventory and the scales measuring aspects of the individual's evaluation of the recovery programme.

Multiple difficulties of a theoretical sort attend upon what has been referred to as the second aspect of Affect Balance. Various difficulties are present which the research has not addressed. One concerns what the nature of a theory of Affect Balance might be. Making sense of a theory amounts to interpreting its language. This necessitates having language that refers to or is grounded in a model or aspects of reality. To be an empirical theory it must be dependent on experience. Two sorts of problem arise here, about the epistemic status of any proposed theory of Affect Balance. People can give honest statements about psychological states to which they are necessarily unique observers - the explanatory value of such self reports, though, is not dependent upon the honesty with which they are made. The reports may or may not be accurate or determine people's feelings and behaviour. Such individuals may not be aware of important influences upon them of a covert or unconscious sort. Even the assumption that mind is a product of the individual has been questioned recently by philosophers. Mind and emotion in a social constructionist view of mind embodies the idea that the fundamental psychological reality emerges from social interaction: what was public becomes private. In this connection I have noted that the higher order concept of hedonic tone is not amenable to scrutiny as the product of a single mental state; quite how it would be as a social episode is bewildering. Just as important for the conception of Affect Balance offered in this thesis is the description of the psychological resource referred to as the second aspect of

Affect Balance. The term is used here as a noun the substance of which is assumed to exist as an aspect of the individual's psychological reality. In this thesis only an elementary description has been offered.

Future research in this area needs to occur on two fronts at once. Firstly, the elucidation and clarification by conceptual analysis of the basic assumptions necessary for a multifaceted theory of Affect Balance of the sort introduced in this thesis is required. Secondly more detailed empirical exploration of the model needs to be undertaken to test out some of the suggestions raised in the thesis regarding the utility of the measure and model in treatment research and social science research. This would additionally provide or fail to provide convergent validity for a theory of Affect Balance.

APPENDIX A

CORRELATION MATRIX

(phase_no = 1)

Correls.: SEV_DEP		DRIE	ACC_	PCC_	OCC_	SCC_
SEV_DEP	1.0000	.2930	.5030**	.4488**	.2220	.5024**
DRIE	.2930	1.0000	.2780	.1377	.1542	.3643*
ACC_	.5030**	.2780	1.0000	.5501**	.5224**	.5674**
PCC_	.4488**	.1377	.5501**	1.0000	.5051**	.5194**
OCC_	.2220	.1542	.5224**	.5051**	1.0000	.4087**
SCC_	.5024**	.3643*	.5674**	.5194**	.4087**	1.0000
DCC_	.4240**	.1671	.5965**	.4165**	.3611*	.7161**
HCC_	.0837	.0470	.1292	-.0954	-.0881	-.0811
SELF_EST	-.3194*	-.3746*	-.4622**	-.4213**	-.2671	-.5255**
PLUS_LF
MINUS_LF
LF_TOTAL
REL_CXC
INT_CXC
HLP_CXC
NETTOT	-.3891**	-.2778	-.4997**	-.2832	-.1870	-.4081**
NETTOT4	-.3669*	-.2474	-.4617**	-.2421	-.1411	-.3729*
NETTOT5	-.3942**	-.2942	-.5148**	-.3090*	-.2204	-.4241**

Correls:	DCC_	HCC_	SELF_EST	PLUS_LF	MINUS_LF	LF_TOTAL
SEV_DEP	.4240**	.0837	-.3194*	.	.	.
DRIE	.1671	.0470	-.3746*	.	.	.
ACC_	.5965**	.1292	-.4622**	.	.	.
PCC_	.4165**	-.0954	-.4213**	.	.	.
OCC_	.3611*	-.0881	-.2671	.	.	.
SCC_	.7161**	-.0811	-.5255**	.	.	.
DCC_	1.0000	-.0022	-.6477**	.	.	.
HCC_	-.0022	1.0000	.1654	.	.	.
SELF_EST	-.6477**	.1654	1.0000	.	.	.
PLUS_LF	.	.	.	1.0000	.	.
MINUS_LF	1.0000	.
LF_TOTAL	1.0000
REL_CXC
INT_CXC
HLP_CXC
NETTOT	-.4229**	-.0007	.3852*	.	.	.
NETTOT4	-.4005**	.0080	.3756*	.	.	.
NETTOT5	-.4270**	-.0083	.3795*	.	.	.

N of cases: 61 1-tailed Signif: * - .01 ** - .001

Continued:

Correls:REL_CXC INT_CXC HLP_CXC NETTOT				NETTOT4	NETTOT5	
SEV_DEP	.	.	.	-.3891**	-.3669*	-.3942**
DRIE	.	.	.	-.2778	-.2474	-.2942
ACC_	.	.	.	-.4997**	-.4617**	-.5148**
PCC_	.	.	.	-.2832	-.2421	-.3090*
OCC_	.	.	.	-.1870	-.1411	-.2204
SCC_	.	.	.	-.4081**	-.3729*	-.4241**
DCC_	.	.	.	-.4229**	-.4005**	-.4270**
HCC_	.	.	.	-.0007	.0080	-.0083
SELF_EST3852*	.3756*	.3795*
PLUS_LF
MINUS_LF
LF_TOTAL
REL_CXC	1.0000
INT_CXC	.	1.0000
HLP_CXC	.	.	1.0000	.	.	.
NETTOT	.	.	.	1.0000	.9777**	.9828**
NETTOT49777**	1.0000	.9221**
NETTOT59828**	.9221**	1.0000
N of cases: 61				1-tailed Signif: * - .01 ** - .001		

(phase_no = 3).

Correls:	SEV_DEP	DRIE	ACC_	PCC_	OCC_	SCC_
SEV_DEP	1.0000
DRIE	.	1.0000	.5290**	.4265**	.3646*	.5461**
ACC_	.	.5290**	1.0000	.6087**	.4234**	.6447**
PCC_	.	.4265**	.6087**	1.0000	.3481*	.5066**
OCC_	.	.3646*	.4234**	.3481*	1.0000	.4485**
SCC_	.	.5461**	.6447**	.5066**	.4485**	1.0000
DCC_	.	.3996**	.6378**	.5166**	.4802**	.6161**
HCC_	.	.1597	.3266*	.3651*	.3711*	.2397
SELF_EST	.	-.5060**	-.5940**	-.5177**	-.3075*	-.5122**
PLUS_LF	.	-.1962	-.0727	-.1654	-.0111	-.1114
MINUS_LF	.	.2069	.1878	.0433	.2254	.2248
LF_TOTAL	.	-.0613	.0228	-.1054	.0840	.0091
REL_CXC	.	.0619	.0030	-.0823	.1210	.0918
INT_CXC	.	.0261	-.0497	-.0552	.0469	.0681
HLP_CXC	.	.0438	-.0183	-.0202	.0133	.0622
NETTOT	.	-.4030**	-.4146**	-.2578	-.1067	-.3330*
NETTOT4	.	-.3630*	-.3669*	-.2094	-.0600	-.2836
NETTOT5	.	-.4284**	-.4461**	-.2929	-.1433	-.3675*

Correls:	DCC_	HCC_	SELF_EST	PLUS_LF	MINUS_LF	LF_TOTAL
SEV_DEP
DRIE	.3996**	.1597	-.5060**	-.1962	.2069	-.0613
ACC_	.6378**	.3266*	-.5940**	-.0727	.1878	.0228
PCC_	.5166**	.3651*	-.5177**	-.1654	.0433	-.1054
OCC_	.4802**	.3711*	-.3075*	-.0111	.2254	.0840
SCC_	.6161**	.2397	-.5122**	-.1114	.2248	.0091
DCC_	1.0000	.4105**	-.5486**	-.0502	.1998	.0444
HCC_	.4105**	1.0000	-.1149	-.0106	.0419	.0092
SELF_EST	-.5486**	-.1149	1.0000	.1611	-.2402	.0216
PLUS_LF	-.0502	-.0106	.1611	1.0000	.4576**	.9314**
MINUS_LF	.1998	.0419	-.2402	.4576**	1.0000	.7498**
LF_TOTAL	.0444	.0092	.0216	.9314**	.7498**	1.0000
REL_CXC	.0534	.0581	.1636	.2032	.0177	.1584
INT_CXC	.1798	.1370	.0834	.0865	.0710	.0934
HLP_CXC	.1582	.1792	.1079	.0515	.0485	.0582
NETTOT	-.3059*	.1305	.5018**	.1155	-.2307	-.0085
NETTOT4	-.2553	.1563	.4736**	.1563	-.1850	.0406
NETTOT5	-.3420*	.1066	.5155**	.0795	-.2642	-.0490

N of cases: 61

1-tailed Signif: * - .01 ** - .001

Continued:

Correls:	REL_CXC	INT_CXC	HLP_CXC	NETTOT	NETTOT4	NETTOT5
SEV_DEP
DRIE	.0619	.0261	.0438	-.4030**	-.3630*	-.4284**
ACC_	.0030	-.0497	-.0183	-.4146**	-.3669*	-.4461**
PCC_	-.0823	-.0552	-.0202	-.2578	-.2094	-.2929
OCC_	.1210	.0469	.0133	-.1067	-.0600	-.1433
SCC_	.0918	.0681	.0622	-.3330*	-.2836	-.3675*
DCC_	.0534	.1798	.1582	-.3059*	-.2553	-.3420*
HCC_	.0581	.1370	.1792	.1305	.1563	.1066
SELF_EST	.1636	.0834	.1079	.5018**	.4736**	.5155**
PLUS_LF	.2032	.0865	.0515	.1155	.1563	.0795
MINUS_LF	.0177	.0710	.0485	-.2307	-.1850	-.2642
LF_TOTAL	.1584	.0934	.0582	-.0085	.0406	-.0490
REL_CXC	1.0000	.5627**	.5451**	.3661*	.4074**	.3249*
INT_CXC	.5627**	1.0000	.9424**	.2434	.2486	.2343
HLP_CXC	.5451**	.9424**	1.0000	.2356	.2452	.2231
NETTOT	.3661*	.2434	.2356	1.0000	.9874**	.9913**
NETTOT4	.4074**	.2486	.2452	.9874**	1.0000	.9580**
NETTOT5	.3249*	.2343	.2231	.9913**	.9580**	1.0000

N of cases: 61

1-tailed Signif: * - .01 ** - .001

(phase_no = 7).

Correls:	SEV_DEP	DRIE	ACC_	PCC_	OCC_	SCC_
SEV_DEP	1.0000	.1788	.3370*	.3940**	.1467	.4259**
DRIE	.1788	1.0000	.2604	.3504*	-.0216	.4816**
ACC_	.3370*	.2604	1.0000	.6418**	.4234**	.5289**
PCC_	.3940**	.3504*	.6418**	1.0000	.1868	.4650**
OCC_	.1467	-.0216	.4234**	.1868	1.0000	.3334*
SCC_	.4259**	.4816**	.5289**	.4650**	.3334*	1.0000
DCC_	.3830*	.3800*	.6597**	.5715**	.3323*	.6490**
HCC_	-.1420	-.1305	.1954	.0371	.2558	-.1461
SELF_EST	-.2244	-.3624*	-.5961**	-.5527**	-.1311	-.6025**
PLUS_LF
MINUS_LF
LF_TOTAL
REL_CXC
INT_CXC
HLP_CXC
NETTOT	-.2315	-.2155	-.4642**	-.2858	-.1942	-.4214**
NETTOT4	-.1937	-.2189	-.4300**	-.2689	-.1510	-.3909**
NETTOT5	-.2620	-.2102	-.4891**	-.2976*	-.2299	-.4436**

Correls:	DCC_	HCC_	SELF_EST	PLUS_LF	MINUS_LF	LF_TOTAL
SEV_DEP	.3830*	-.1420	-.2244	.	.	.
DRIE	.3800*	-.1305	-.3624*	.	.	.
ACC_	.6597**	.1954	-.5961**	.	.	.
PCC_	.5715**	.0371	-.5527**	.	.	.
OCC_	.3323*	.2558	-.1311	.	.	.
SCC_	.6490**	-.1461	-.6025**	.	.	.
DCC_	1.0000	-.0368	-.8179**	.	.	.
HCC_	-.0368	1.0000	.0571	.	.	.
SELF_EST	-.8179**	.0571	1.0000	.	.	.
PLUS_LF	.	.	.	1.0000	.	.
MINUS_LF	1.0000	.
LF_TOTAL	1.0000
REL_CXC
INT_CXC
HLP_CXC
NETTOT	-.6028**	.1251	.6610**	.	.	.
NETTOT4	-.5811**	.1356	.6412**	.	.	.
NETTOT5	-.6153**	.1147	.6712**	.	.	.

N of cases: 61 1-tailed Signif: * - .01 ** - .001

Continued:

Correls:REL_CXC INT_CXC HLP_CXC NETTOT NETTOT4 NETTOT5

SEV_DEP	.	.	.	-.2315	-.1937	-.2620
DRIE	.	.	.	-.2155	-.2189	-.2102
ACC_	.	.	.	-.4642**	-.4300**	-.4891**
PCC_	.	.	.	-.2858	-.2689	-.2976*
OCC_	.	.	.	-.1942	-.1510	-.2299
SCC_	.	.	.	-.4214**	-.3909**	-.4436**
DCC_	.	.	.	-.6028**	-.5811**	-.6153**
HCC_1251	.1356	.1147
SELF_EST6610**	.6412**	.6712**
PLUS_LF
MINUS_LF
LF_TOTAL
REL_CXC	1.0000
INT_CXC	.	1.0000
HLP_CXC	.	.	1.0000	.	.	.
NETTOT	.	.	.	1.0000	.9936**	.9951**
NETTOT49936**	1.0000	.9775**
NETTOT59951**	.9775**	1.0000

N of cases: 61 1-tailed Signif: * - .01 ** - .001

APPENDIX B

MULTIPLE ANALYSIS OF VARIANCE TABLES (MANOVA)

ALCOHOL VARIABLES (Phase 1 and 7)

(phase_no = 1).

EFFECT .. NEWAB4

Multivariate Tests of Significance

(S = 7, M = -1/2, N = 22 1/2)

Test Name	Value	Approx. F	Hypoth. DF	Error DF	Sig. of F
Pillais	1.04811	1.33330	49.00	371.00	.075
Hotellings	1.57111	1.45201	49.00	317.00	.032
Wilks	.28170	1.40583	49.00	243.03	.050
Roys	.44683				

Univariate F-tests with (7,53) D. F.

Variable	Hypoth. SS	Error SS	Hypoth. MS	Error MS
A_ALC_US	71.85298	599.88472	10.26471	11.31858
B_ALC_US	94.65831	1038.25972	13.52262	19.58981
C_ALC_US	75.21910	1450.55139	10.74559	27.36889
D1_ALC_U	647.09000	3895.92639	92.44143	73.50805
D2_ALC_U	94.75895	380.45417	13.53699	7.17838
G_ALC_US	1668.45189	9007.35139	238.35027	169.95003
SEV_DEP	2083.61243	9017.14167	297.65892	170.13475

Variable	F	Sig. of F
A_ALC_US	.90689	.508
B_ALC_US	.69029	.680
C_ALC_US	.39262	.903
D1_ALC_U	1.25757	.289
D2_ALC_U	1.88580	.090
G_ALC_US	1.40247	.224
SEV_DEP	1.74955	.117

(phase_no = 7).

EFFECT .. NEWAB4

Multivariate Tests of Significance

(S = 7, M = -1/2, N = 20 1/2)

Test Name	Value	Approx. F	Hypoth. DF	Error DF	Sig. of F
Pillais	.84609	.96242	49.00	343.00	.549
Hotellings	1.12578	.94854	49.00	289.00	.575
Wilks	.37871	.95804	49.00	222.73	.557
Roys	.31693				

Univariate F-tests with (7,49) D. F.

Variable	Hypoth. SS	Error SS	Hypoth. MS	Error MS
A_ALC_US	80.67143	659.32857	11.52449	13.45569
B_ALC_US	91.18162	850.95873	13.02595	17.36650
C_ALC_US	461.75850	2284.27659	65.96550	46.61789
D1_ALC_U	376.52824	3748.73492	53.78975	76.50479
D2_ALC_U	15.05163	698.45714	2.15023	14.25423
G_ALC_US	1051.13043	12573.0099	150.16149	256.59204
SEV_DEP	3243.56435	9794.36548	463.36634	199.88501

Variable	F	Sig. of F
A_ALC_US	.85648	.547
B_ALC_US	.75006	.631
C_ALC_US	1.41503	.221
D1_ALC_U	.70309	.669
D2_ALC_U	.15085	.993
G_ALC_US	.58521	.765
SEV_DEP	2.31816	.040

PSYCHOLOGICAL VARIABLES (Phases 1, 3 and 7)

(phase_no = 1).

EFFECT .. NEWAB4

Multivariate Tests of Significance

(S = 7, M = -1/2, N = 22 1/2)

Test Name	Value	Approx. F	Hypoth. DF	Error DF	Sig. of F
Pillais	1.13240	1.46122	49.00	371.00	.029
Hotellings	1.58632	1.46608	49.00	317.00	.029
Wilks	.26475	1.48414	49.00	243.03	.028
Roys	.39475				

Univariate F-tests with (7,53) D. F.

Variable	Hypoth. SS	Error SS	Hypoth. MS	Error MS
ACC_	259.11664	575.80139	37.01666	10.86418
PCC_	180.75257	397.50972	25.82180	7.50018
OCC_	128.21623	551.22639	18.31660	10.40050
SCC_	232.10729	700.12222	33.15818	13.20985
DCC_	167.91637	390.01806	23.98805	7.35883
HCC_	117.11760	527.24306	16.73109	9.94798
SELF_EST	5844.06120	25704.2667	834.86589	484.98616

Variable	F	Sig. of F
ACC_	3.40722	.004
PCC_	3.44282	.004
OCC_	1.76113	.115
SCC_	2.51011	.026
DCC_	3.25976	.006
HCC_	1.68186	.134
SELF_EST	1.72142	.124

(phase_no = 3).

EFFECT .. NEWAB4

Multivariate Tests of Significance

(S = 6, M = 0, N = 21 1/2)

Test Name	Value	Approx. F	Hypoth. DF	Error DF	Sig. of F
Pillais	1.06827	1.54723	42.00	300.00	.021
Hotellings	1.69860	1.75253	42.00	260.00	.005
Wilks	.26688	1.66146	42.00	214.52	.011
Roys	.50177				

Univariate F-tests with (6,51) D. F.

Variable	Hypoth. SS	Error SS	Hypoth. MS	Error MS
ACC_	220.84674	835.22222	36.80779	16.37691
PCC_	29.55292	429.82639	4.92549	8.42797
OCC_	64.62835	474.88889	10.77139	9.31155
SCC_	141.50192	406.72222	23.58365	7.97495
DCC_	205.93103	526.00000	34.32184	10.31373
HCC_	64.94947	479.32639	10.82491	9.39856
SELF_EST	9949.50192	18698.2222	1658.25032	366.63181

Variable	F	Sig. of F
ACC_	2.24754	.053
PCC_	.58442	.741
OCC_	1.15678	.344
SCC_	2.95722	.015
DCC_	3.32778	.008
HCC_	1.15176	.347
SELF_EST	4.52293	.001

Continued:

(phase_no = 7).

EFFECT .. NEWAB4

Multivariate Tests of Significance

(S = 7, M = 1/2, N = 21)

Test Name	Value	Approx. F	Hypoth. DF	Error DF	Sig. of F
Pillais	1.25003	1.20777	63.00	350.00	.149
Hotellings	2.24470	1.50665	63.00	296.00	.013
Wilks	.19754	1.34497	63.00	253.92	.058
Roys	.58435				

Univariate F-tests with (9,50) D. F.

Variable	Hypoth. SS	Error SS	Hypoth. MS	Error MS
ACC_	195.53452	585.86548	21.72606	11.71731
PCC_	73.71865	321.68135	8.19096	6.43363
OCC_	79.70198	544.48135	8.85578	10.88963
SCC_	145.80198	295.84802	16.20022	5.91696
DCC_	267.72698	273.00635	29.74744	5.46013
HCC_	68.52500	553.80833	7.61389	11.07617
SELF_EST	14807.5651	15593.1683	1645.28501	311.86337

Variable	F	Sig. of F
ACC_	1.85418	.081
PCC_	1.27315	.275
OCC_	.81323	.606
SCC_	2.73793	.011
DCC_	5.44812	.000
HCC_	.68741	.717
SELF_EST	5.27566	.000

SOCIAL VARIABLES (Phases 1 and 7)

(phase_no = 1).

EFFECT .. NEWAB4

Multivariate Tests of Significance

(S = 7, M = 0, N = 22)

Test Name	Value	Approx. F	Hypoth. DF	Error DF	Sig. of F
Pillais	1.24954	1.41241	56.00	364.00	.034
Hotellings	1.90560	1.50698	56.00	310.00	.016
Wilks	.21799	1.47719	56.00	253.03	.024
Roys	.44595				

Univariate F-tests with (7,53) D. F.

Variable	Hypoth. SS	Error SS	Hypoth. MS	Error MS
AVAT	83.37439	167.41250	11.91063	3.15873
ADAT_PCE	11225.9531	38549.0306	1603.70758	727.34020
AVSI	74.54629	595.09306	10.64947	11.22817
ADSI	193.98934	685.65000	27.71276	12.93679
DISP	1003.70280	1973.37917	143.38611	37.23357
DIS_B	1224.70701	3104.53889	174.95814	58.57621
SUPP_P	49.02828	298.77500	7.00404	5.63726
SUPP_R	26.02999	214.92083	3.71857	4.05511

Variable	F	Sig. of F
AVAT	3.77071	.002
ADAT_PCE	2.20489	.048
AVSI	.94846	.478
ADSI	2.14217	.055
DISP	3.85099	.002
DIS_B	2.98685	.010
SUPP_P	1.24245	.297
SUPP_R	.91701	.501

(phase_no = 7).

EFFECT .. NEWAB4

Multivariate Tests of Significance

(S = 8, M = 0, N = 20 1/2)

Test Name	Value	Approx. F	Hypoth. DF	Error DF	Sig. of F
Pillais	1.32933	1.10711	72.00	400.00	.271
Hotellings	2.38533	1.36659	72.00	330.00	.037
Wilks	.17837	1.22477	72.00	269.14	.128
Roys	.59083				

Univariate F-tests with (9,50) D. F.

Variable	Hypoth. SS	Error SS	Hypoth. MS	Error MS
AVAT	79.48056	187.11944	8.83117	3.74239
ADAT_PCE	17271.3079	28125.6254	1919.03422	562.51251
AVSI	193.19722	662.45278	21.46636	13.24906
ADSI	244.28175	861.90159	27.14242	17.23803
DISP	898.90675	1090.07659	99.87853	21.80153
DIS_B	1077.60556	1856.57778	119.73395	37.13156
SUPP_P	11.71905	204.61429	1.30212	4.09229
SUPP_R	22.08810	133.16190	2.45423	2.66324

Variable	F	Sig. of F
AVAT	2.35977	.026
ADAT_PCE	3.41154	.002
AVSI	1.62022	.135
ADSI	1.57457	.149
DISP	4.58126	.000
DIS_B	3.22459	.004
SUPP_P	.31819	.965
SUPP_R	.92152	.515

APPENDIX C

STEPWISE MULTIPLE REGRESSION

```
REGRESSION
/VARIABLES (COLLECT)
/STATISTICS HISTORY
/DEPENDENT DRIE
/METHOD STEPWISE G_ALC_US SEV_DEP SELF_EST AVAT ADAT_PCE ADSI AVSI
DIS_B SOC_P SOC_B SUPP_R NETTOT4 AGE.
```

* * * * M U L T I P L E R E G R E S S I O N * * * *

Listwise Deletion of Missing Data

Equation Number 1 Dependent Variable.. DRIE

Summary table

Step	MultR	Rsq	F(Eqn)	SigF	Variable	BetaIn
1	.4069	.1656	32.543	.000	In: SELF_EST	-.4069
2	.4504	.2029	20.745	.000	In: NETTOT4	-.2053
3	.4788	.2292	16.058	.000	In: AGE	-.1650

* * * * M U L T I P L E R E G R E S S I O N * * * *

Equation Number 1 Dependent Variable.. DRIE

----- Variables in the Equation -----

Variable	B	SE B	Beta	T	Sig T
SELF_EST	-.08599	.01904	-.33117	-4.515	.0000
NETTOT4	-.11416	.04756	-.17809	-2.400	.0175
AGE	-.98280	.41788	-.16496	-2.352	.0199
(Constant)	16.48300	1.35918		12.127	.0000

```

REGRESSION
/VARIABLES (COLLECT)
/STATISTICS HISTORY
/DEPENDENT SEV_DEP
/METHOD STEPWISE SELF_EST DRIE AVAT ADAT_PCE ADSI AVSI SOC_P
SOC_B SUPP_R NETTOT4 AGE.

```

* * * * M U L T I P L E R E G R E S S I O N * * * *

Listwise Deletion of Missing Data

Equation Number 1 Dependent Variable.. SEV_DEP

Summary table

Step	MultR	Rsqr	F(Eqn)	SigF	Variable	BetaIn
1	.3058	.0935	16.922	.000	In: NETTOT4	-.3058

----- Variables in the Equation -----

Variable	B	SE B	Beta	T	Sig T
NETTOT4	-.43495	.10573	-.30583	-4.114	.0001
(Constant)	31.87991	1.20673		26.418	.0000

```
REGRESSION
/VARIABLES (COLLECT)
/STATISTICS HISTORY
/DEPENDENT ACC_
/METHOD STEPWISE SELF_EST DRIE AVAT ADAT_PCE ADSI AVSI G_ALC_US DIS
SOC_P SOC_B SUPP_R PCC_ TO HCC_ NETTOT4 AGE.
```

* * * * M U L T I P L E R E G R E S S I O N * * * *

Listwise Deletion of Missing Data

Equation Number 1 Dependent Variable.. ACC_

Summary table

Step	MultR	Rsq	F(Eqn)	SigF	Variable	BetaIn
1	.6685	.4468	132.475	.000	In: DCC_	.6685
2	.7105	.5047	83.063	.000	In: PCC_	.2657
3	.7442	.5539	67.045	.000	In: NETTOT4	-.2374
4	.7644	.5844	56.594	.000	In: G_ALC_US	.1890
5	.7758	.6018	48.366	.000	In: DIS_B	.1577
6	.7848	.6158	42.482	.000	In: AGE_	-.1208
7	.7925	.6280	38.108	.000	In: OCC_	.1243

* * * * M U L T I P L E R E G R E S S I O N * * * *

Equation Number 1 Dependent Variable.. ACC_

----- Variables in the Equation -----

Variable	B	SE B	Beta	T	Sig T
DCC_	.35824	.07517	.31124	4.766	.0000
PCC_	.26350	.07017	.20827	3.755	.0002
NETTOT4	-.08150	.02188	-.20003	-3.725	.0003
G_ALC_US	.04141	.01392	.15765	2.974	.0034
DIS_B	.06775	.02536	.15482	2.671	.0083
AGE_	-.52577	.18985	-.13884	-2.769	.0063
OCC_	.14103	.06200	.12427	2.274	.0243
(Constant)	3.27547	.82168		3.986	.0001

```

REGRESSION
/VARIABLES (COLLECT)
/STATISTICS HISTORY
/DEPENDENT ADAT_PCE
/METHOD STEPWISE G_ALC_US SEV_DEP DRIE SELF_EST AVSI ADSI DISP DIS_
SOC_B SOC_P SUPP_P SUPP_R NETTOT4 PCC_ OCC_ SCC_ DCC_ HCC_ AGE ACC_
          * * * * M U L T I P L E   R E G R E S S I O N   * * * *

```

Listwise Deletion of Missing Data

Equation Number 1 Dependent Variable.. ADAT_PCE

Summary table

Step	MultR	Rsq	F(Eqn)	SigF	Variable	BetaIn
1	.5058	.2559	56.387	.000	In: ADSI	.5058
2	.5384	.2899	33.266	.000	In: NETTOT4	.1879

* * * * M U L T I P L E R E G R E S S I O N * * * *

Equation Number 1 Dependent Variable.. ADAT_PCE

----- Variables in the Equation -----

Variable	B	SE B	Beta	T	Sig T
ADSI	3.21622	.46065	.46963	6.982	.0000
NETTOT4	.59645	.21348	.18793	2.794	.0058
(Constant)	19.47114	4.61183		4.222	.0000


```

REGRESSION
/VARIABLES (COLLECT)
/STATISTICS HISTORY
/DEPENDENT SOC_P
/METHOD STEPWISE G_ALC_US SEV_DEP DRIE SELF_EST AVAT ADAT_PCE
AVSI ADSI SUPP_P SUPP_R NETTOT4 PCC_OCC_SCC_DCC_HCC_AGE ACC_.

```

* * * * M U L T I P L E R E G R E S S I O N * * * *

Listwise Deletion of Missing Data

Equation Number 1 Dependent Variable.. SOC_P

Summary table

Step	MultR	Rsq	F(Eqn)	SigF	Variable	BetaIn
1	.3307	.1093	20.133	.000	In: NETTOT4	-.3307
2	.3625	.1314	12.328	.000	In: SELF_EST	-.1578

* * * * M U L T I P L E R E G R E S S I O N * * * *

Equation Number 1 Dependent Variable.. SOC_P

----- Variables in the Equation -----

Variable	B	SE B	Beta	T	Sig T
NETTOT4	-.14195	.03970	-.27733	-3.575	.0005
SELF_EST	-.03271	.01608	-.15777	-2.034	.0436
(Constant)	8.80584	.86448		10.186	.0000

```
REGRESSION
/VARIABLES (COLLECT)
/STATISTICS HISTORY
/DEPENDENT WINCODE
/METHOD STEPWISE G_ALC_US SEV_DEP DRIE SELF_EST AVAT ADAT_PCE AVSI
DIS_B SOC_P SOC_B SUPP_R NETTOT4 ACC_ TO HCC_ AGE.
```

* * * * M U L T I P L E R E G R E S S I O N * * * *

Listwise Deletion of Missing Data

Equation Number 1 Dependent Variable.. WINCODE

Summary table

Step	MultR	Rsq	F(Eqn)	SigF	Variable	BetaIn
1	.2434	.0592	10.325	.002	In: AGE	.2434
2	.2994	.0896	8.025	.000	In: ADAT_PCE	.1774

* * * * M U L T I P L E R E G R E S S I O N * * * *

Equation Number 1 Dependent Variable.. WINCODE

----- Variables in the Equation -----

Variable	B	SE B	Beta	T	Sig T
AGE	.54969	.19805	.21097	2.776	.0062
ADAT_PCE	.01567	6.71368E-03	.17739	2.334	.0208
(Constant)	1.96115	.56544		3.468	.0007

DECLARATION

I hereby declare that this thesis has been composed by myself and that all research relating to the thesis has been done solely by the author.

Signed

Brian Coyle